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The Boundaries of Science

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## THE BOUNDARIES OF SCIENCE



# THE BOUNDARIES OF SCIENCE

*A Study in the  
Philosophy of Psychology*

by  
JOHN MACMURRAY

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## PREFACE

THIS book is based upon the Deems Lectures on 'The Philosophy of Psychology' which I had the honour to deliver in the University of New York in 1936. The present study has, however, a narrower scope than those lectures. The Deems Trust requires its lecturers to deal with the relation of science to religion. The original lectures were, accordingly, designed to compare the analysis of human motives in modern psychology with the very different treatment of human motivation which is inherent in Christianity; and to determine the relation between the scientific and the religious points of view. In the book I have omitted the reference to religious psychology, partly because my treatment of it in the lectures seemed inadequate, and partly because the philosophical

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study of scientific psychology led to conclusions which demanded separate treatment.

This limitation of theme, and the modifications and additions which it has involved, make it impossible to represent the book as the publication of my Deems Lectures. Nevertheless, its relation to those lectures is so close that I wish to acknowledge, with gratitude, the debt that I owe to the Deems trustees and to the University of New York.

JOHN MACMURRAY

*University College, London*

*30th March 1939*

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## Chapter I

### THE PHILOSOPHY OF SCIENCE

The creation of science is the act of Western civilization in its modern period. It is the peculiar contribution of Western civilization to the development of human life. The creation of science defines the cultural unity of our Western world from the time of the Renaissance to the present day. Other civilizations in other periods have made different contributions to the history of man, perhaps in some cases more important contributions. But science is our gift to the world. There are large grounds for thinking that the process of the production of science is nearing completion, and with it the completion of the main function of what we have grown accustomed to calling modern society. Already the outlines of a new task for which a new form of society is required are taking shape before our

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eyes, and no exceptional foresight is required to guess that the new society will be defined by the discovery of the use of science for essentially human ends.

To recognize that science is the act of our modern period is easy. To understand what it implies is difficult. It obviously does not mean that the production of science is the only achievement of modern society, nor that it is even the main interest and the main effort of the period. It does seem to me that it is the only continuity of satisfactory and successful achievement which runs through the whole period, and which, once completed, can survive the dissolution of the form of social experience which produced it. Our effort in producing science need never be repeated, but remains for all time, available to the whole of humanity in future ages. It means also that the multifarious energies and efforts of the period are constrained by the pressure of the historic conditions under which they are exercised to contribute, usually unconsciously and often unwillingly, to this achievement. In the result, when we look back over the cultural history of the times, it is possible to use the production of science as the focus of our understanding and to see how the

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other characteristic activities of life are modified and shaped towards the accomplishment of this supreme task.

Our Western philosophy, in the modern period, is in this way orientated towards the production of science. The relation between modern philosophy and modern science is intimate, though not usually direct. Above all, we must guard against the tendency to imagine that the conclusions of science become or can become the starting-point of philosophy. So far as philosophy busies itself with science, it is concerned to understand what the scientist is doing and how he does it; to estimate the validity of the general principles which the scientist takes for granted, and to discover what light the success of various sciences in their particular field can throw upon more general problems which science itself does not attempt to answer. Science can never form the basis of a philosophy because the questions of philosophy are wider than the questions of science, and science itself falls in a certain sense within the field of philosophy. A philosophy which was based upon the conclusions of science would be like a church standing on its steeple, and it would not stand very long. A philosophy of science may be possible. A

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science of philosophy would be completely meaningless. What happens in the modern period to philosophy is that it plays its part in shaping and in expressing the general attitudes of mind and will which are necessary to the social achievement of the task of producing science. This may be brought about by the fact that philosophers are themselves interested in science and themselves competent and creative in some department of scientific work. But it may be achieved equally well if they are interested in other aspects of human activity under the pressure of the social conditions which constrain the society to which they belong to produce science. The expressing and moulding of an ethical outlook or a political attitude may be just as essential to the production of science as a consideration of science itself. More important, perhaps, than any other factor is the general limitation and concentration of interest and attention upon the field of experience within which lies the major problem which society faces in a particular period. The major practical problems of a society tend to determine a general focus for attention and interest throughout the society, and even where a thinker reacts most strongly against this limitation of atten-

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tion he must still be preoccupied with it in his effort to be rid of it. What we find, in fact, is not so much that the philosophers are preoccupied with science, but that philosophers and scientists alike tend to share a common set of preoccupations and a common framework of presuppositions and ideas. Thus there is a parallelism between the stages of the development of modern science and of modern philosophy, which is in essentials as clearly marked in the philosophers who are not directly interested in science as in those who are. But it is in science that this characteristic temper and quality of the modern period finds its most effective and lasting expression.

The reason for thinking that the task of creating science is nearing completion is the appearance of scientific psychology in our own time. The process of establishing science has proceeded by stages which are determined by the range of its subject-matter. In the first stage the establishment of the physical sciences is the immediate task. Not until the sciences that are concerned to investigate the behaviour of matter have been effectively organized is it possible to bring the behaviour of living organisms within the range of scientific methods of investiga-

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tion. The production of physical science is followed by the extension of science into the field of life, and this second stage is completed in the nineteenth century by the discovery of the satisfactory method of carrying out the task of biology. In turn, the establishment of the biological sciences leads to a further extension of scientific enquiry into the field of human behaviour. That this third stage is a final stage is clear from the fact that no further extension of the field of investigation is possible. All the phenomena of experience are in principle covered by these three sets of sciences. Once the psychological sciences have been set upon a sure footing the task of producing science will be complete, though not of course the task of science itself. It is the creation of science, not the completion of the work of science, that is the act of our modern society.

Now the stages of the development of modern society itself correspond with this development of science and are connected with it. This, again, does not mean that science and modern society are related as cause and effect. Rather the reverse. It is the development of society that conditions the development of science. The tendency to think of modern society as the

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creation of science is a curious inversion of the truth. Science is clearly the creation of modern society. But there is a real sense in which the stages of modern social development are conditioned by the task of producing science. This statement is of course a simplification, like all general statements, and must be interpreted with due care. A more precise formulation would indicate that science is the instrument created by modern society to solve the problems set for it by the conditions of its life. The ultimate purpose is to solve these urgent practical problems. The necessity of finding their solution ultimately determines the effort to produce science. But this in turn necessitates these modifications of social habit and social structure which are demanded if the production of science is to proceed to completion. In other words, the same ultimate pressures which determine the stages of the production of science determine the stages of the development of modern society and determine the relation between these two developments. For our purpose we need only note the fact that one of the essential requirements for the production of science is the production of an attitude of mind which can express itself in scientific investigation. The production

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of science is itself the expression of an attitude of mind and a direction of interest which is not usual in society. It was with difficulty that European society was prevailed upon to tolerate scientific investigation; and each stage in the development of science had to meet and overcome a strong resistance before it could establish itself and find acceptance. Until there was sufficient support in the general outlook of society for the scientific adventure, science could never be established, however much certain individuals might desire or endeavour to undertake the task. A preparation of the mind of society for the toleration of scientific effort was necessary. In this task of adapting the temper and habits of society, stage by stage, to the toleration and support of the process of establishing science modern philosophy has played an important and not always a conscious role. It has at least provided the general means of systematic expression for those social attitudes of mind which condition the stages of the development of science. Thus, in the first period which fills the time between the break-up of mediæval society and the democratic revolutions, the problem of science is a material problem which is solved by the use of mathematical



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methods, while the philosophy has a markedly mathematical structure and talks in terms which are clearly derived from a preoccupation with the same field. In the second period which runs from the democratic revolutions to the World War, the scientific task is the production of biology, while philosophy concerns itself with the notion of organism and uses categories and forms of thought which clearly have their origin in a preoccupation with the phenomena of life and growth.

It is in this sense alone that we are justified in describing modern philosophy as the philosophy of science. It is the philosophy of a period in which the minds of reflective individuals are inevitably conditioned by the social preoccupations and problems which find their resolution in the development of science. This is of course not the usual meaning of the phrase. In our conventional usage the philosophy of science refers only to the direct efforts of philosophers to deal with the philosophical problems which science presents, and it is in this sense that we shall be concerned as we proceed with the philosophy of psychology. We shall set ourselves the task of dealing with the philosophical problems which are presented by the final stage of the

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production of science when it enters the field of human behaviour. But for the moment it is helpful to recognize that there is a general sense in which the whole of modern European philosophy might be considered as a philosophy of science. It might also be advisable to consider another sense in which the term might be used. There is in our own day a widespread attitude of mind which is itself a kind of immanent philosophy and which is often referred to as the scientific view of the world, or more simply as scientific materialism. It should be recognized that this 'philosophy of science' is neither scientific nor philosophical. It is rather in the nature of a popular ideology or of a reaction to the impact and prestige of scientific achievement upon the minds of intelligent people who are neither scientists nor philosophers. It is not scientific for two reasons. The first is that it is in no sense the result of any scientific investigation carried out by scientific methods. There is no text-book of science in which it could find any legitimate place. The second is that it is not necessarily the view taken by scientists. Eminent scientists may explicitly repudiate it without any damage to their effectiveness as scientists. On the other hand, it is not a view which

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is the result of any philosophical discussion of science nor a view which is held by trained philosophers, however sympathetic they may be to science itself. It is the product of an uncritical response to the success of science in its own field, a response which is rather emotional than intellectual, and which substitutes Science, vaguely personified, as an object of faith and worship for the God of religion. This scientific materialism is rather a theology of science than a philosophy, and it will wither under the white light of reason even more rapidly than the theology which it displaces.

The philosophy of science, in a strict sense, is the philosopher's attempt to evaluate science itself. This does not mean that he attempts either to justify or to condemn science. Such an effort is, indeed, absurd. Science needs no justification and by its very existence and development it refutes any condemnation. This view of science requires to be enforced in the opening chapters of the present book. We start from the conviction that modern science represents one of the finest achievements of human activity and constitutes a final and unassailable value in human history. To condemn science is as absurd as to condemn architecture. To justify science is to

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gild the lily. Let this be said once and for all, so that it need not be repeated. I yield to no one in my admiration of modern science, and my belief in it is not a reaction to the practical effects that have so far followed from the use to which science has been put. On the whole, it seems to me that modern society has been characteristically inept in the use that it has made of the science it has created. But even if modern society uses its science to destroy itself, I should still refuse to retract by one iota my assertion that science is in itself wholly admirable and quite indispensable to the achievement of a truly human society. I insist on this, because a good deal of what I have to say about psychology might seem at first sight to involve not merely a disparagement of psychology but of science as a whole. I shall indeed have occasion to reject some of the claims which are made for science by its uncritical devotees. But that is a totally different matter. I am not concerned to maintain that science is a good religion.

Moreover, this postulate of our inquiry, that science neither needs justification nor permits of condemnation, extends over the whole field of possible scientific investigation and includes, therefore, the scientific investigation of human

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behaviour. I am a whole-hearted believer in the value and importance of scientific psychology, and I am convinced that our civilization stands in jeopardy mainly because it has so little faith in science and is in danger of losing what little faith it had.

But a faith in science does not involve an uncritical acceptance of the conclusions of particular sciences as eternal truth. Such an acceptance is entirely unscientific. All scientific conclusions are liable to revision, and it is by their perpetual revision that science lives. To treat any scientific conclusion as final is to turn it into a dogma, and at once it ceases to be scientific and becomes part of a system of dogmatic belief. Science is not a body of ascertained truth, even where its basis is a formidable body of ascertained fact. A scientific theory is never itself a fact, but the interpretation of facts in the light of a hypothesis. Science itself is the never-ending process of the revision of beliefs, consequently it can have nothing to do with beliefs which are not considered liable to revision. To turn a scientific theory into a dogma is to remove it from the field of scientific investigation. When we do this we are offending against science itself, and becoming obscurantist.

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It may be maintained, however, that this only applies to the conclusions of particular sciences, not to the general conclusions of science as a whole. The answer to this is that there are no such general conclusions because there is no such thing as science as a whole. Science is only a set of particular sciences. If any conclusion is to be scientific it must be one of the conclusions of a particular science. A conclusion is scientific only if it is reached by scientific methods and stands in a definite relation to a body of theory which has been built up by the use of these methods. We must guard against the tendency to use the term 'scientific' as a substitute for 'true'. A belief may be true even if it is not scientific. It may be scientific and yet untrue. This tendency is part of the 'religion' of science to which I have referred above. It comes from our natural, but very unscientific hankering after an authority which will certify the truth of our beliefs. But science depends upon the repudiation of such authority and upon our ability to overcome our natural tendency to seek for such an illusory certitude.

There are no doubt a number of beliefs which a large number of scientists tend to hold in common, and the fact that they do so strongly

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suggests that this is because they are scientists. Such beliefs are not, therefore, scientific. We may divide them, for our present purpose, into those which they find useful or necessary for the purposes of their scientific investigations and those which are suggested by the general conclusions and hypotheses of the branch of science with which they are occupied. Neither of these types of belief is scientific in the strict sense. Those of the first type are postulates of science. They are assumptions which a particular science finds it necessary or helpful to make; but they are not themselves the product of scientific investigation, and they are not based upon the kind of evidence, or subject to the type of verification which science demands; and if their truth is questioned there is no science which could undertake the investigation of their validity. But they do form part of the general body of systematic belief that constitutes the content of the science in question; and the success of the science in its own field must have some bearing upon their truth. On the other hand, the general beliefs which scientists tend to hold about the world in general because they are scientists, are neither conclusions of science nor involved in the systematic structure of science. They are, in

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the strict sense, prejudices; and they are the natural prejudices of minds that are preoccupied with a limited field of experience. They are, indeed, likely to be less trustworthy than the philosophical prejudices of the average intelligent man. Prejudices are not necessarily untrue, they are merely uncritical. The evidence for their truth has not been examined. They are the reactions of our minds to the general pressure of our experience. We are too ready to disparage prejudices which conflict with our own and, equally, too ready to cherish our own as sacred truths. The great bulk of any man's beliefs consists of prejudices, and we could not live without them. The real truth about all opinions which are the 'result of experience' is that they are the product of a limited experience. I should myself be very chary of challenging the prejudices of an experienced mountaineer about the proper organization of an Alpine climbing expedition. But my feeling that he was likely to be right would not extend to his views on modern painting. And when it comes to matters of general concern I should be less inclined to trust the prejudices of any one whom I knew to be one-sided in his interest, because one section of his experience is heavily



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weighted against the rest and is likely to tell too heavily in the production of his views on matters of general concern. It is for this reason that the views of the scientist on matters of general experience which fall outside the scope of his particular science gain no special right to consideration because they are the opinions of a scientist. Rather they are less likely to be trustworthy because of his continuous preoccupation with a limited field of experience and the consequent tendency to give more weight to that section of experience than to the rest. 'The common man', as Mr. Chesterton once said, 'may not know very much, but he is not usually very one-sided.' Views which are not the result of deliberate and critical examination are more likely to be trustworthy where the kind of experience which gives rise to them is well balanced. Specialization tends to load the dice.

Philosophy is largely concerned with the criticism and examination of prejudice. It seeks, by whatever methods are available, to rid those reactions of our minds to our experience as a whole which take the form of general opinions about the nature of the world, from the one-sidedness which is the result of the weighting of the evidence through the inevitable limitation

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of the experience on which they rest. It seeks to substitute for mere natural reaction to experience a judgement which is deliberate, and which because of its deliberateness is fully aware of the reasons for holding these opinions rather than others. Without the natural prejudices which emerge from the pressure of experience, it would have nothing to work upon. But it must approach them with suspicion and guard against taking them at their face value. One of the main elements in this suspicion is the recognition that it is always probable that the experience on which they rest is too limited, and that it needs to be enlarged. General conclusions must not be based on specialized evidence. In the philosophy of science, we are primarily concerned with the critical examination of those general views, those natural prejudices which are either embodied in the assumptions of science or which result from preoccupation with science; but which are not themselves the results of scientific investigation in the sense that they are not the product of investigation carried out by the methods which science uses. When I define the philosophy of science as the philosopher's evaluation of science itself, it is this that I have in mind. Philosophy cannot under-

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take to determine the scientific correctness of strictly scientific conclusions. That is the business of science. But it can undertake the task of estimating the weight that should be attributed to the conclusions of science in the formation of our general views about the nature of the world. When an eminent scientist, like Professor Eddington, writes a book which attempts to estimate what contribution physical science has made to our knowledge of the real nature of the physical world, he is not writing science, but philosophy. The views which he expresses are not reached by the use of the methods of physical science, in which he is a master, but by some of the methods which philosophers are accustomed to employ. His conclusion, that physical science can tell us little if anything of the nature of the physical world, does not impress the philosopher in the way that any of his scientific conclusions undoubtedly would. What does impress him is the astonishing fact that a scientist should hold such views. The arguments for them which Professor Eddington advances are arguments with which he is already familiar and to which he knows the objections. And in disputing the conclusion, if he does dispute it, he is on his own ground, and the fact that he is

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not a scientist puts him at no disadvantage. If Professor Eddington has the better of the argument, it will be not because he is a good scientist but because he is the better philosopher. The philosopher will take Professor Eddington's views for what they are, a contribution to the philosophy of science, and his natural attitude to them will be not that they are likely to be right because Professor Eddington is a scientist, but that they are likely to be inadequate because they depend upon too narrow a preoccupation with physical science itself. The philosophy of science requires to be based upon considerations of the relation of scientific experience to other aspects of experience. It is probable, here, that a physicist will give too little weight to the conclusions of biological or psychological science, and that the scientist will give too little weight to experience of the physical world which is not the result of scientific investigation.

This may be enough to suggest what the business of a philosophy of science is. It is not a scientific task. The philosophy of science is not concerned at all with the scientific validity of scientific theories. But it is concerned with the business of relating science as a whole to the other parts and aspects of common human ex-

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perience. It has to attempt to integrate science with aspects of experience which are not scientific, remembering that science itself depends for its very existence upon natural beliefs which are not and cannot be reached by scientific methods, however much they may be modified or clarified by the results of scientific investigation. The philosophy of psychology, with which we are to be concerned, is part of the general philosophy of science. It is the part which has a special interest for philosophers, for various reasons. In the first place, it is in this field that science is seeking to establish itself in our own time, and so far with only partial success. The effort is already having a profound effect upon our natural prejudices about ourselves, and these effects have in turn important practical consequences. They tend to undermine the basis of traditional and customary belief upon which social life, as well as individual life, has hitherto been based. This effect is not dependent upon the validity of the results achieved. It is the effort to achieve a scientific psychology which, by suggesting fundamental doubts about our customary notions, undermines the stability of our moral and social prejudices. It produces a widespread lack of confidence in the articles

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of faith by which previous generations have lived, without substituting any effective alternative. In the second place the attempt to establish science in the psychological field encounters peculiar difficulties, and the nature of these difficulties throws light upon the philosophical problem of the relation of science to other departments of human experience. From a scientific point of view, the most satisfactory sciences are the physical sciences. This is no doubt due in part to the fact that the physical sciences are older, but it is also due to the fact that it is easier to apply scientific methods of investigation in the physical field. This is no doubt one of the main reasons why physical science was earlier established. The difficulty of establishing science in the field of psychological phenomena is not merely due to the fact that it is only recently that the attempt has been seriously made. It is due even more to the peculiar nature of the field of study. For this reason it is the field of psychological science which is most instructive for the philosophy of science. One of the large questions which we have to ask about science is how far the methods of scientific investigation can be carried, or, to put the question in a more familiar form, 'Are

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there limits to scientific knowledge, and if so what are they?' Most of the great philosophers of the modern world have come to the conclusion that there must be, and they have reached this conclusion by an examination of the assumptions and the methods which are common to all scientific inquiries. These answers, however, even in spite of their unanimity, are open to doubt, because the methods of examination which are open to philosophy are not themselves exempt from criticism. It is always possible to reply that these philosophical conclusions are abstract and speculative, and that the question whether there are limits to scientific knowledge can only be answered in the effort to extend the limits of scientific investigation. This is a view which I myself should endorse. However much I might be convinced on philosophical grounds that there must be departments of experience in which the questions which arise cannot be settled by science, I should still insist that the scientist cannot accept these conclusions as limiting his effort to extend the field of science. At the end of the eighteenth century Kant came to the conclusion, on philosophical grounds, that any inquiry, so far as it is scientific, must necessarily be mathematical. If scientists had

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accepted this conclusion at its face value, it would probably have prevented the effort to establish the biological sciences. There is a sense in which the conclusions of a philosophy of science require to be verified by the process of scientific development. If a philosopher comes to the conclusion that there is something that science cannot do, and later on science does it, the philosopher's theory is refuted and so far his philosophy of science demands to be reconstructed; and if a philosopher reconstructs his philosophy of science he will almost inevitably have to reconstruct the whole of his philosophy in consequence. It is probably true that one of the reasons why most modern philosophers have set limits to science has been a belief that in the field of human behaviour at least the effort to establish science was *a priori* ruled out. The qualified success of the attempt to do so already challenges disbelief, and for that reason alone makes the consideration of the philosophy of psychology imperative. Even if the traditional view of the philosopher proved to be true after all, it would still be necessary to restate it in terms of what the psychological sciences are doing. For they are doing something, by the use of scientific method, and the results, what-



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ever they may mean, are already there. The question that faces the philosophy of science is this: 'How far do the psychological sciences, by their existence, refute or confirm or demand a modification in our philosophy of science?'

In the third place, there is the fact, to which I have already drawn attention, that once science is established in the psychological field, it is in principle complete. There is no further field to which it could be extended. The grouping of the sciences into physical, biological and psychological covers the whole field of phenomena. The philosopher, therefore, is at last presented with a science which is in principle complete, for his examination and evaluation. Hitherto he has had to be content with evaluating an incomplete science, and therefore to speculate about what science would be when it had penetrated into the psychological field if it ever did so. So far as there are difficulties inherent in the nature of science from the philosophical point of view, they are certain to reveal themselves in the psychological sciences in a way that they do not necessarily reveal themselves in the restricted fields of physics and biology. Not until the psychological sciences have come into existence and are actually functioning, is

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the philosopher in a satisfactory position to attempt an answer to the ultimate question of the philosophy of science, 'What does science, now that we have achieved it, really amount to?'

It is for these reasons that I have been enticed into the effort to undertake a preliminary investigation of the philosophy of psychology. It is not intended to be more than preliminary. It is limited to the attempt to discover the peculiar difficulty that faces the psychological sciences, to illustrate it in certain selected portions of the field, and to draw certain *prima facie* conclusions of a philosophical character. By the psychological sciences I mean the whole group of sciences which have for their field of investigation the various aspects of human behaviour. The philosophy of psychology consists, therefore, in the philosophical examination of the sciences of human behaviour. The general difficulty which I have selected for examination, because it seems to me to be the root of all special difficulties, is this. In the psychological sciences, science is reflected back upon itself, and becomes part of its own field of study. For science, in all its branches, is a human activity. Because the establishing and carrying on of science is

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itself a part of human behaviour, it is necessarily a part of the subject-matter of the sciences of human behaviour. If science is to complete the conquest of the whole field of phenomena, it must produce a scientific theory of itself. The philosophical question which arises can then be stated in this form, 'Can science give a satisfactory scientific account of science as part of a general scientific account of human behaviour?' If it cannot, then the limits of science have been reached in the development of science itself. If it can, then certain very important conclusions must follow concerning the nature of human knowledge, of which philosophy must take account. It is with various aspects of this question that we shall be concerned in the chapters which follow.

## Chapter 2

### THE SOCIOLOGY OF SCIENCE

We can begin our study best by considering sociology, in which the problem reveals some of its most general characteristics. Sociology is one of the psychological sciences. It is concerned with human behaviour in its social aspect. It is an attempt to study social behaviour scientifically through considering the forms taken at different times by human groups, to trace the development of those forms and to determine if possible what laws human societies obey in their behaviour and in their development. It is natural to suppose that the social habits of human groups are not haphazard. They must have certain understandable relations to the conditions and circumstances in which human life is lived. There are certain general conditions which apply universally to human groups.

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There are other conditions which are more or less variable. If a society is to exist at all it must conform to the natural conditions of human existence which are set for it by its environment. There is at least this element of necessity in human life and to a considerable extent it must determine the forms of social behaviour which are possible in that environment. Sociology can therefore seek to determine scientifically what these environmental conditions are and how they necessitate certain forms of social behaviour. It can seek to trace correlations between changes in the conditions of group-life and changes in the form of social behaviour. In particular its task will be to determine characteristic forms of social behaviour and to relate them to characteristic conditions which determine their presence. Forms of behaviour which are characteristic of all human groups will then be correlated with universal conditions which apply to all social life, variable characteristics will be correlated with variable conditions. It is the effort to produce a science of society that makes this necessary. The mere description of the various forms of social behaviour that are open to examination is not itself science. It is at most the preliminary col-

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lection of data upon which the attempt to construct a sociological theory must be based. If there is to be a scientific sociology we must postulate that there is some kind of necessity which determines the forms that we actually observe; and the business of the science is to discover what this necessity is and to determine the laws by which it operates. The idea that there can be a science of sociology itself involves the postulate that the forms of social behaviour are not arbitrary, not matters of chance, but are determined; and that the characteristic forms of social behaviour in any society are necessarily related in some manner, to conditions which give rise to them.

Now, in pursuing its task, there must come a point at which sociology has to consider and account for the development of science. Science is a characteristic activity of the Western societies of the modern period. Indeed, there is a sense in which it is *the* characteristic social activity of modern society in the West. Any scientific account of the forms of social behaviour which characterize the contemporary social life of the West which did not concern itself with science, would be obviously inadequate. On the other hand, science is from the sociological point of

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view, a unique characteristic of our contemporary societies. The social effort to produce science is certainly not a general characteristic of all human group-life. The sociology of science would therefore be an attempt to correlate the rise and development of science with the special conditions of human life in Europe and America during the last four or five centuries. If there is to be a sociological account of science it must postulate that the appearance of science at the Renaissance and its development from stage to stage in our own society to the present day, is not a matter of chance but of necessity, and that this necessity must reside in the special social conditions of Western life during this period. If this is denied, the possibility of a sociology of science is denied in principle, and in that case limits are set to the possibility of science. In this field at least a scientific knowledge would then be impossible, and we should have to ask what made it impossible in this field and why it was possible in other fields.

We cannot pre-judge the issue by assuming that a sociology of science is impossible, and it is only in the attempt to produce one that the question of its impossibility or possibility could be settled. It is certainly possible to make the

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attempt. We can determine the questions which a sociology of science must ask and the kind of answers which it must give to these questions. There are two main questions which have to be answered. First, what are the social conditions which give rise to science, and which determine its development? Second, what are the effects of science upon the social behaviour of the society in which it develops?

The simplest way forward will be to make an attempt to suggest an answer to these sociological questions. We may do this with reasonable impunity, since we are only concerned with the form that a sociology of science must take, and not with the scientific validity of any particular theory. Let us then raise the question of the origins of science from the sociological point of view. Science had its origin in European society at the end of the Middle Ages. It was, indeed, one of the most important results of the break-up of mediæval society. The conditions which make possible the development of science may be divided into two sorts, the inner, subjective or psychological conditions, and the outer, objective, material conditions. The material conditions are no doubt mainly economic. They are pressures of social necessity—the necessity



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which society finds itself under of adapting its forms of social behaviour to changing material conditions of life, and certain new possibilities of doing so. In general, the account which will have to be given will take the general form of an effort to show how the environment, working through the basic necessities of social life, began to pose new problems which the society had to answer. If there is to be a sociology of science, then we must assume that the appearance of science is not a matter of chance but that it is a social response to a new demand from the environment. It is part of the effort of society to adapt itself to changes in the conditions of its existence. It is always the pressure of necessity which sets a problem for society. Science was the answer of modern society to the problems set for society by environmental conditions at that point in history. A good deal of work has been done of a sociological kind in this effort to explain changes of social behaviour by correlating them with changes in the economic conditions of life. What is called the economic interpretation of history is, in fact, an attempt to establish a general sociology in terms of the material conditions of social life and their changes. There is, therefore, no need for us to

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pursue this aspect of the question further. We know the kind of answer that an economic interpretation of history would have to give to the question about the origins and development of science as a social phenomenon.

But such an account would not be complete without a treatment of the inner conditions which must be present before science can arise and develop. As it is this aspect of the question which throws most light upon the philosophy of the psychological sciences we must treat it a little more fully. The attitude of mind which was characteristic of the Middle Ages could not have produced science. It had no interest in doing so. Mediæval society did not want scientific questions asked and answered because it had no intention of modifying the traditional forms of social life. Its interests were conservative. No society will really be interested in getting certain questions answered unless they have a practical importance of some kind. No doubt natural curiosity at all times makes us ask questions and seek to answer them merely for the sake of doing so. But such a natural curiosity will never give rise to science or indeed to any sustained effort of serious thought that is characteristic of a particular society. Some practical

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necessity of asking these particular questions in these particular circumstances must provide the incentive. The mediæval world was a traditional society. It lived by custom and habit. It assumed, therefore, that the right way to do anything was the traditional way, the way it had always been done. Its intentional life was conservative. This reference to intention is important, because science is an intentional activity. Scientific knowledge cannot happen to people. It can only result from a deliberate effort and the deliberate effort must be a social one because science, unlike art, is only possible through a continuity of co-operation. In other words, the inner condition of science is the intention to produce it, and the intention to produce science is a particular aspect of the intention to achieve progress. Sociologically, therefore, the inner condition of the appearance of science as a feature of the life of a particular society is that the society should have formed the intention of progress. In the mediæval world this intention was absent. The intentional life of society was concerned with the maintenance of custom and authority. Before society can produce science there must be a socially effective group of people who have abandoned the out-

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look that finds its canons of rightness in the wisdom of the past, and replaced it by an attitude which is determined to make the future better than the past. Until people feel that the right way to do things is to do them better than they have ever been done, the idea of progress and the intention to progress are unthinkable. Science is necessarily a function of a progressive society. It is, indeed, the reflective aspect of a progressive intention. It can only occur in a society which feels that it can live its social life better than its predecessors, and is prepared to make the attempt. The sardonic conservative no doubt will ask, 'How do you propose to set about it?' To that the primary answer must be: 'We don't know, but we are going to find out.' The effort to find out is the production of science.

Science is, thus, bound up with the practical intention of changing social habits of life. It is indeed this aspect of it that constitutes its challenge to traditionalism and produces the opposition of the conservative forces in society. It is no sociological account of science which relates it to the general human desire for knowledge. That desire is universal in human nature and operates at all times in all societies. But it does

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not produce science at all times and in all societies. And since the actual people who produce science are limited in number to a relatively small minority, the social motive which underlies the production of science is not *their* motive necessarily. That is why a sociological account must find the inner conditions for the production of science in a general social motive which is not necessarily the motive which animates particular scientists. Their motives may be the same as those which animated the mediæval alchemist or the mediæval theologian. The question sociology has to answer is in terms of the social pressures which direct the individuals desire for knowledge in the special direction which results in science rather than in theology. The necessity of changing the social habits of life, if society is to survive or maintain its standard of living, is not in itself sufficient, since it might be met by a conservative resistance to change which actually did result in the failure to survive or to maintain the standard of life. If a society meets the threat by an effort of conscious adaptation instead of by an increased effort to maintain the customary forms of adaptation, then it will need a new sort of knowledge on which to base its deliberate effort. From a

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sociological point of view, it is inadmissible to explain the development of science without reference to the field of practical effort. If, however, we recognize this, we can mark the necessity for recognizing inner as well as outer conditions by noticing that people will never set out to discover things unless they have first become convinced of their own ignorance. A doubt of the validity of the old forms of knowledge is a precondition of the effort to produce new ones. This psychological condition we must relate to the external condition by reminding ourselves that people will never discover that they do not know something until they have discovered that they cannot do something which they desire to do. Equally, they will never discover that they cannot do something unless they want to do something different from what they are accustomed to do. The social condition for the origin of science is that a particular society wants to strike out upon a new path in its social behaviour instead of to maintain its traditional form of behaving. Towards the end of the Middle Ages in western Europe this was the situation which arose, and it marks a very significant change in the inner conditions of social life. Large numbers of people found themselves

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wanting to live their lives in their own way instead of in the traditional and customary way. They began to break loose from the social custom and to attempt to live by their own judgement. The social result was the threat of chaos. The first discovery was, in fact, that people could not live their own lives in their own way without first discovering how to do it. This discovery no doubt tended to throw numbers of people back into the conservative camp as conscious opponents of the effort to invent new ways of living. But it also gave rise, in other quarters, to an effort of a reflective kind directed towards the achievement of the kind of knowledge that would enable the effort to succeed. Thus the desire to devise a new form of social life led to the desire to create a new kind of knowledge as the condition of success. The progressive intention which had arisen in society was thus canalized into the field of reflection. The first stage in the transition from a customary to a progressive social life had to be a deliberate effort to achieve progress in knowledge. Science is the first stage of the effort to achieve social progress, and it is the first stage, because until science has been developed, it is in fact impossible to change practical modes of social

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life without disastrous consequences. This limitation of progress to the field of reflection means that while the effort to produce science is going on, the practical side of social life must continue to be governed for the most part by tradition and custom, though the hold of traditional authority over the life of thought must be broken. The limitation of progress to the theoretical field is at the same time a limitation of customary authority to the practical field.

This change in the attitude to life which occurred at the end of the Middle Ages, and which gave rise, as its first effective expression, to the production of science, constitutes perhaps the most far-reaching of all revolutions in human society. It involved the production and spread of a radically new attitude to life, and for this reason it is fundamentally a change in moral outlook. Its novelty does not consist in the fact that numbers of people rebelled against customary authority. That has always happened to a greater or less extent. It consists rather in the association of the sense of rightness with the revolt. Instead of feeling that the right way to live was the way that was hallowed by tradition and custom and guarded by social authority, they began to feel that the right way



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was to live better than anybody had lived before. This involved the feeling that their own way of living was not as good as it might be, and that the beliefs which held their minds were not as true as they might be. In this way the ideas of duty and of practical goodness ceased to sustain custom and became associated with experiment. People began increasingly to realize their ignorance and their limitations, and to feel that the proper mode of human life lay in the effort to change their customary habits of life and thought in order to achieve higher and better forms both of life and of knowledge.

Such an account of the origin of science as a social phenomenon is still too general. It does not account for the particular character of the new effort after knowledge which constitutes modern science. It does not answer the question why it takes the form of a mathematical inquiry into the behaviour of matter, which seeks to formulate its results as laws of change. A thorough-going sociology of science would require to answer this question in detail, and its ability to do so would be one of the evidences that it was working along the right lines. But for our present purpose a detailed account is

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unnecessary. We are concerned only with the general form which the sociological inquiry must take. No doubt the outer conditions, which set the problem, can throw a good deal of light upon the question. The development of mathematics is no doubt sociologically related to the development of the technique of the counting house. Capitalism itself rests upon the development of mathematical techniques; and the practical effort towards new modes of life depended, as much as science, upon their use. The large-scale production of commodities for a widespread market is only possible if the qualitative variety of goods can be measured by a common and universal standard. Qualitative differences must be reduced to quantitative terms if they are to be commensurable in the market. All commodities must be represented as complexes of identical units of value, and equations established between different complexes. Thus the way in which the merchant succeeds in dealing with his world of materials is identical in principle with the way in which the physicist deals with the material world. There is thus an instructive set of correlations to be worked out between the methods of developing science and the methods of developing capitalism; between

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the enterprising spirit of modern society, in the practical and in the reflective fields. Apart from all controversial questions about causal relations between them, they clearly belong together in their social setting, and provide the raw material for the investigations of sociology.

But we must not allow ourselves to be drawn aside into such fascinating side-issues. We should merely observe that the underlying social intention of progress makes it inevitable that the new reflective effort should be directed primarily upon the material world in an effort to secure a conscious and deliberate control of material conditions. The control of material is the primary necessity of life, and an improvement in our control of matter is the primary necessity if we are to improve our ways of social life. The first effective expression of the intention to progress must be an effort to achieve the basic means to progress; and this is clearly an increasing mastery of the material world. Further, the first necessary step in extending our mastery of the material world is to achieve the kind of understanding of it on which control can be based. From the sociological point of view science is clearly the necessary first stage in the social effort to progress. It is the effort to dis-

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cover how the means of life can be controlled for human purposes. The social motivation of science is the desire (or rather the intention) to secure control of the external world.

For purposes of exposition we have, of course, been simplifying the social situation; and certain qualifications are necessary. We have drawn the contrast between the mediæval world and the modern world very sharply, as an opposition between a progressive and a conservative intention. In fact, what occurred was the appearance, in a limited number of people and in a limited field, of the progressive intention under external conditions which allowed it to be socially effective. The great majority of people over the whole field of life and all of them over a large part of the field, remain subject to custom and authority. The novelty in the social situation is the effective intrusion of the progressive intention into a social motivation which remains predominantly conservative. One of the more significant features of modern social history is the progress of the progressive intention itself both in respect of the numbers of the individuals in whom it works and of the range of the field to which it is applied. Even for an individual to break loose from his customary

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modes of behaviour is very difficult, and to break with social customs requires a great deal of courage. For a society to do this requires almost the courage of despair, and it will do so only under strong compulsion. The effort must have been accompanied by anxiety, and the desire for progress must have generated a corresponding fear of progress. Every step in advance, had, therefore, to face and overcome the fear of making it. Fear, in all its forms, is a negative motive. It inhibits action and, therefore, is the defender of custom and tradition. Progress and, therefore, science as the reflective aspect of progress is only possible through the overcoming of social inhibitions. Every new step in social process is impossible until the desire for change in a particular field has become strong enough in society to overcome the social inertia of fear; and the most fundamental fear in human life is the fear of changing the structural habits of society. A society can only change its way of life deliberately if this inhibition has been overcome. The development of science thus depends upon overcoming social inhibitions. It is this general psychological factor which explains the struggle that science has had to wage throughout the history of its develop-

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ment with the forces of social conservatism. It accounts also for one of the major peculiarities of modern society, its insistence on dissociating progress in scientific knowledge from the control of social development. It is easier to overcome the fear of change in the field of thought than in the field of practical activity. Up to a point, at least, it is possible to change our beliefs without changing our practical habits; and, therefore, the forces that inhibit the desire for progress are more easily overcome in the field of knowledge than in the field of action. Under the stress of the tension between the desire for progress and the fear of abandoning custom and tradition modern society achieved a compromise by which the effort to achieve progress in scientific knowledge was socially accepted while any effort to apply it to the transformation of social practice was declined. This is the sociological meaning of the notion that knowledge should be sought for its own sake. In practice, of course, it is not possible to maintain the separation. The new knowledge is taken advantage of; clandestinely, as it were. But by making 'pure' knowledge an ideal, we do succeed in slowing down the rate of change in social practice which science produces; and we make the

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application of science to social life haphazard and uncontrolled. The changes in social habit which result are unco-ordinated, and lead to dislocation and breakdown in the mechanism of social life.

There is a significant parallel between the broad stages in the development of modern society and the stages of the development of science. The first period, which runs from the end of the Middle Ages to the period of the democratic revolutions is the period in which the physical sciences are established. The second period is the period of the Romantic movement and the development of democracy which we may conveniently consider closed by the World War of 1914-18. This is the period in which the biological sciences are established. The effort to create a scientific psychology belongs to the contemporary phase of social development. The detailed correlation of these stages of scientific progress with the corresponding stages of social progress in the practical field would be part of the task of a sociology of science. We need only suggest its relation to the change in the inner conditions which we have singled out for emphasis. We have recognized already that the primary stage is necessarily the effort to under-

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stand the material world. But there is an inner reason why it is inevitable that the physical sciences should be the first to arise. The material world is less highly charged with emotion than the field of organic or personal life. The social inhibitions which prevent the investigation of the material world are consequently more easily overcome. The mind will resist any interference with its customary attitudes to the world in the field of biology or psychology much more strongly than in the field which physics studies. Since the psychological condition for the development of science is the overcoming of these inhibitions, it was inevitable that the demand for progress in knowledge should find its primary outlet in the field of physical science. In that field the resistance to change was weakest, and the development of mathematical physics was the line of least resistance.

Even in this field the resistance was considerable, and it took a long time for physical science to win acceptance for itself in modern society. But by the middle of the eighteenth century western Europe had acquired sufficient confidence to go further. The overcoming of social resistance to objective investigation in the material field naturally weakened the forces of



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resistance on the whole. The power of custom and authority had been sufficiently reduced to enable society to apply new methods to the field of life. The drive towards progress in the reflective field invaded the world of organic phenomena. Behind the Romantic movement there lies a general shift of interest from matter to Nature. The new idea of Nature is concerned with those features of experience which are peculiar to the life of organisms, such as growth and reproduction. It is possible to trace this movement of thought and interest from the aesthetic field to the philosophical, and from the philosophical to the scientific; from its first expressions in a highly emotional and imaginative idealism to the matter-of-fact observation and realism of evolutionary biology. In this second stage, the crisis in the effort to overcome social inhibition was the resistance to the theory of natural evolution. The Darwinian theory which marks the success of the effort towards a scientific biology was widely resisted as a challenge to the traditional religious belief in the creation of the world by God. It is easy to see now that the resistance was not really religious but drew its strength from the fear of looking at the facts and so challenging the dignity of hu-

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man life. When that resistance had been generally overcome, and the idea of biological evolution had been accepted, what had happened was that the strength of customary belief and the fear of progress had been sufficiently overcome in European society to enable people generally to look the facts of biology in the face. It is only within living memory that this has been generally accomplished. It has meant the beginning of the third stage in the development of science. The field which is most highly charged with emotion and in which the inhibitions which express our fear of the facts are most powerful, is the field of personal life. Our society is now beginning to be able to look directly at the facts about ourselves, our actual behaviour and its motivation. This is the inner social condition which is necessary for the development of psychological science. As a result, it is no longer impossible to apply the methods of scientific investigation to the study of human behaviour. The final field of phenomena is being invaded by science. Accounts of the motivation of human behaviour are being produced which steadily tend to replace the traditional views about how we behave and why we behave as we do; and these new scientific descriptions show that our

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customary conceptions have largely been the expression of our own vanity and our desire to think well of ourselves.

We have noticed that one of the main characters of modern society is the limitation of the progressive effort to the theoretical field. Broadly speaking, the tension between the progressive intention and the fear of progress resulted in a compromise which gradually removed the inhibitions which prevented scientific inquiry in one field after another, while holding on to traditional methods and purposes in the practical field. We decided, if I may use the personification, to face the task of deliberately developing our knowledge of how to control the world but not to proceed to the length of deliberately controlling it. We decided, in other words, that the new knowledge must be pursued as an end in itself, but it must not look beyond knowledge to the control of practical life. This involves leaving the control of the practical activities of society, whether in the political or the economic field, to men of practical common sense, who act in terms of custom and precedent, by means of a traditional technique. This does not, in itself, prevent the development of science from determining changes in the practical life

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of society. It merely prevents the society as a society from determining rationally the use which is made of the new knowledge. We have to notice now that this feature of modern society is itself the expression of a social inhibition. This inhibition defends the customary motivation of social practice, and confines the effort of progress to the external or objective field. An interesting and instructive expression of it is to be found in the conviction of the innate goodness of man which works so strongly in the thought of the early Romantics at the beginning of the democratic period. When Rousseau, for example, opens his famous treatise on the Social Contract, he proposes 'to take men as they are, and States as they ought to be'. He is pleading for a new and revolutionary step in social progress. Yet he takes it for granted that no change is required in the subjective motivation of human behaviour, and that only an external change in the environmental conditions is required. The effect of this is to keep the attention even of the progressive mind, upon the external condition of social practice. The organization of life, its organic aspect, is to be altered, but not its inner or personal aspect which determines the direction of effort. This aspect is taken as fixed by

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nature, and at the same time, as inherently good. Here the inhibition reveals itself in a concealed antinomy. If the inner constitution of human nature is fixed, it cannot be either good or bad. It is a mere matter of fact. To hold at once that it is fixed and that it is subject to valuation can only mean that one recognizes the possibility of variation in it while rationalizing a resistance to any attempt to alter it. Now, this inhibition prevents any scientific inquiry into the motivation of human behaviour and, therefore, it prevents the establishment of the psychological sciences. The acceptance by society of the effort to establish a scientific psychology is evidence that this inhibition is being broken down. The completion of the process of the development of science itself involves an inner change in the conditions of social behaviour. We should notice that it is not the application of scientific psychology that is here in question, but the possibility of making the inquiry at all whatever its conclusions may be. A social inquiry into the motivation of social behaviour is itself evidence of a profound modification in social motivation. So long as science confines its inquiries to the external field, the question of the deliberate social effort to pro-

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gress remains in abeyance. The issue between conservative and radical intentions remains theoretical. The appearance of psychological science as a social phenomenon means necessarily that this situation is coming to an end. Society must, then, decide for or against the deliberate effort to achieve practical progress by applying scientific knowledge. A society which can develop a scientific psychology is a society which has overcome its fear of examining its own motives. It has no longer, therefore, sufficient fear of its own motives to prevent it from doing what it wants to do. The dissociation of theory and practice must come to an end, because there is no longer a sufficiently strong inhibition to maintain it. The completion of the development of science must, therefore, mean the beginning of deliberate social effort to plan and control social behaviour. It must involve a profound revolution in social life.

The structure of modern science is socially conditioned by the inhibition which expresses itself in the idea of knowledge for its own sake. It involves the distinction between subjective and objective factors in experience, and limits the subject-matter of science to the objective field. It is ultimately this that makes science an

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attempt to achieve knowledge of the *external* world. When science penetrates into the field of human behaviour it finds itself in a field in which the distinction between subjective and objective factors breaks down, because the distinction between subjective and objective fields reveals itself as a subjective distinction. We have found ourselves already suggesting, from the sociological angle, that this distinction is itself socially conditioned, that it is necessitated by the peculiar social situation which gives rise to science itself. It does not follow that the appearance of the psychological sciences involves the immediate recognition of the dilemma which is involved. What happens is that the subject-matter which is investigated is selected in terms of the distinction between subjective and objective factors, and it is treated as if it were objective. What is meant here is perhaps more easily comprehensible in examples. Of the psychological sciences the one which is both the earliest to develop and apparently the most advanced is economics. It may seem at first surprising to call economics one of the psychological sciences. It is precisely this surprise which I wish to underline. The psychological sciences are the sciences of human behaviour, and psychology,

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the central science of the group, is now almost universally defined as *the* science of human behaviour. Now it is quite obvious that economics is the science of human behaviour in a particular field and so part of the group of sciences of which psychology is the central science. But it deals with a particular field of human behaviour which is very largely concerned with those activities which are determined by the nature of our relationship to the material world. It is, therefore, relatively easy to represent human behaviour in this field as objectively determined and to discover laws which are relatively independent of the variable factors in the inner conditions of human behaviour. Economic phenomena can then be treated and spoken of as if they were not phenomena of human behaviour but of the external world. Commodities come into the market and disappear from the market. Prices rise and fall. Such statements isolate the phenomena considered from their source in human action, and the laws which are formulated then appear to be external to human beings and of the same character as the laws which are formulated in the sciences of non-human nature. Yet in reality they are quite clearly either the formulations of human habits



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of behaviour or they are nothing. What makes it possible to proceed in economics as if they were not is simply that they are relatively necessary habits imposed upon human life in general by the nature of its environment in general. So far as this is the case, or so far as the sociological structure of the society which is producing the economics is such that it can appear to be the case, it is possible to proceed as if the science of economics were concerned with purely objective phenomena even when in principle we know that this is not strictly the case. But it is part of our problem to understand, from the sociological point of view, why economists feel it necessary to disguise a psychological science in this fashion. For, if there can be a science of sociology, then this aspect of the behaviour of economic science must be socially conditioned, and requires to be understood in terms of its social conditions. It is clearly not a theoretical necessity, and equally clearly the social conditions which give rise to it are not themselves external but psychological conditions.

Sociology itself has shown the same anxiety to maintain a purely 'objective' status. It is this which keeps it so largely concerned with the structure of primitive society. The psychological

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'distance' at which primitive society stands from the society to which the sociologist belongs, makes it appear 'external' to him. In addition, the social habits of primitive society are much more continuous and fixed. There is much less evidence of the modification of traditional modes of behaviour by deliberate thought and effort. It is, therefore, easy to represent these habits as the expression of natural laws which operate independently of the consciousness of human beings, and which operate upon consciousness and are reflected in it. This sociological preoccupation with primitive society may be represented as reasonable because of the simplicity of primitive society and because of a need for beginning at the beginning. But this is rather a rationalization. From the point of view of scientific method it would be much more simple and certain to begin with our own society, which though in a sense more complicated, is yet much simpler to us, since it consists of our own activities and expresses our own habits. The difficulty of observing our own societies scientifically is mainly subjective. There is an inhibition to overcome. The form of science in general is set by the physical and the biological sciences. Our scientific habits are derived

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from them in large part. We tend, therefore, in the psychological field also, to search for an 'external' object of study; an object, that is to say, in whose behaviour we are not implicated, and which is not affected by our processes of thought, or by the knowledge that arises from them. The 'purity' of our knowledge consists, indeed, in its being knowledge of a world which is independent of us; and when we aim at knowledge which is of universal validity the object must be independent of human consciousness in general. The laws of its behaviour must not be subject to modification by human will. If sociology is to be objective or pure science in this sense, then the social behaviour of the groups of human beings which it studies must itself be independent of human will. If it applies itself to the task of understanding the society which produces it, sociology must either maintain that the whole of our social behaviour is independent of our wills, and so is 'unconscious' or automatic—a thesis which is impossible to defend, especially as it must include the denial that the production of science and sociology itself is a deliberate activity—or else it must set limits to the possibility of science by isolating a part of social behaviour which is in principle

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independent of our own intentions as its object of study. The difficulty which would then arise would be the mystery of the relation between the part of our behaviour that is independent of our intention and the part that is dependent upon it. For sociology this relation would necessarily remain incomprehensible.

We need not add to these examples. We shall have occasion later to examine the efforts of psychology to secure an objective and external field of study at some length. At the moment we are only concerned to note the peculiar difficulties which arise in the effort to establish the psychological sciences. From the point of view of the sociologist the social conditions which determine the production of sociology are bound up with the effort of society to control its own organizations and development through knowledge and understanding. The inhibition which has hitherto prevented this is the fear of breaking loose from traditional habits of social life. But, as we saw, it is this same inhibition which resulted in the divorce between the progressive development of knowledge and its application in the practical field. This divorce is responsible for the form of pure science, since it limits progress to the field of theory and makes

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knowledge an end in itself. The possibility of developing scientific sociology shows that this inhibition is weakening and that, therefore, the demand for the deliberate application of knowledge to the control of social development is growing. This puts the sociologists in a dilemma. If sociology is to be a pure science, then the behaviour of society must be represented as independent of human will and intention. Yet a society which can produce a scientific sociology is a society which proposes to control social behaviour by human will and intention.

Some such sociological account of science must form part of a complete science of sociology. The question whether the particular account which we have given is a satisfactory one need not trouble us. It is the necessary form of any sociological account of science as a social phenomenon with which we are concerned. Whatever may be the valid scientific account of the social conditioning of the development of science, the fundamental perplexity will remain. At some point in the attempt to develop sociology the attempt must be made to account for the rise and progress of science in terms of social conditions. If the sociologist makes the attempt to do so, science itself is pinned to its own dis-

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secting table. It no longer stands outside the world of objective phenomena in the external world as pure observer. It is no longer the judge on the bench but the prisoner at the bar; or rather, it has become both at once. For the scientific sociologist science is seen, as it were, through the wrong end of the telescope. It appears, for the first time, as a rather abnormal and accidental phenomenon of social behaviour, peculiar to a relatively short stretch of social history in western Europe. The sociologist sees science as the reflection of his own mode of behaviour, and he sees his own behaviour as a scientist, as a social phenomenon which is conditioned by the peculiar social environment to which he belongs. What then becomes of the claims to a universal knowledge valid at all times for all societies? What can be meant by the claim that it is true in comparison with other theories of the world which have been produced in different societies under different social conditions? How, at least, can its truth be verified? If it is to be verified experimentally, it can only be by the fact that a successful control of social behaviour can be based upon it. But precisely here lies the dilemma, which is not peculiar to sociology but appears in all the psychological

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sciences. If sociological theory can make deliberate social control of social behaviour possible, how can it be true? For its very form implies that social behaviour is determined, not by human intention, but by objective laws which are independent of the will of man.

## Chapter 3

### THE LIMITS OF SCIENCE

With this dilemma of the sociology of science in our minds we may turn with advantage for a little to the philosophical aspect of the problem of the limitations of scientific knowledge. Our sketch of the sociology of science has drawn our attention to the fact that science depends upon our interests, and that these interests are social interests. This in itself implies, in a manner that we have not yet considered, that there are limits to the scope of scientific research. The kind of interest which gives rise to scientific knowledge is not the only kind of interest which we have. There are other interests which give rise to other types of inquiry and to other forms of knowledge. We noticed also that the peculiar social conditions of modern life have made the concentration upon the production of science



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a defining characteristic of the society to which we belong. This helps to explain why modern society through its long concentration upon a social effort for which the achievement of scientific knowledge is essential has come to imagine that the possibilities of science are unlimited. It is the natural result of the preoccupation with the production of science which has been the task of our society during the last few centuries.

Interest is a selective agency and a limitation of interest is one of the conditions of effective action. This itself sets subjective, psychological limits to scientific activity. What we experience depends partly upon how we experience. It is determined, up to a point, by the attitude of mind which we bring to our observation of the world. The reflection of science upon itself in the psychological sciences has the effect of making us aware that science is one of the things we do. Scientific knowledge is dependent upon us, even if the world which is the object of science is not. It depends upon our motives and the activities to which they give rise. That science is concerned with facts we are sufficiently aware. What is now forced upon our notice by science itself is that science depends upon our desires. It depends at least

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upon our desire to observe and understand the facts. Yet what we call the facts themselves depend partly upon our observation and so upon our interest and our attitude of mind in observing. If this is true of the facts themselves, it is more obviously true of our understanding of the facts. Science is not a mere account of our observations. It is the result of our efforts to understand. In a word, science is something that we create, deliberately and purposively. It is what we make it. The purpose for which we create it must determine, to an appreciable extent, how we apprehend the facts, what facts our observation selects as important, and how we deal with these selected facts in order to achieve our purpose. There can be no doubt that science is not independent of the human mind and that, therefore, the character of the knowledge which science provides is partly determined by our own psychology.

Of the modern philosophers, the one who above all others realized the problem that this presents was Immanuel Kant. Sufficient attention has not been given to the fact that Kant belongs to the band of pioneers who created the Romantic movement in Germany. He stands historically, though with a difference, beside

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his contemporaries Lessing, Hamann, Herder, Schiller, and Jacobi. Kant's own sympathy with the Romantic movement is sufficiently shown by his affectionate admiration for Rousseau. One of the main characteristics of the Romantic movement is the insistence upon the spontaneity of the imagination, in knowledge as well as in art. Against the characteristic effort of the first period of modern culture to suppress the colourful fancies of the imagination in favour of the white light of pure reason the new movement claimed that only a creative activity of the imagination could provide that insight into the living reality of nature which deserves to be called knowledge. Like his contemporary pioneers, Kant was convinced that knowledge is created by the spontaneity of the mind, by that productive imagination which he described as a blind art hid in the depths of the soul. To use the language of modern psychology, Kant realized that all our knowledge, including especially our scientific knowledge, is the product of fantasy. His greatness consisted in the fact that he realized the problem which this recognition involves. If we invent our knowledge, what right have we to call it knowledge? For knowledge is by definition the receptivity and not the spon-

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taneity of the mind. The pure spontaneity of the mind is art, not science. If science is the creation of the human imagination, how can it be more than a modern mythology?

That science is the product of human imagination is certainly true from the psychological point of view. Consider, for example, the part played by mathematics in the physical sciences. The physicist is not content until his discovery can be expressed in a mathematical formula. Now, scientific formulae do not grow on gooseberry bushes. They are not to be discovered in the external world. Mathematics, from beginning to end, is an invention of the human mind. It belongs to the subjective field. In so far as physics consists in a set of mathematical formulae, it is something invented by human minds. When scientists describe a natural object as a collection of electrons moving in a field of electrical energy, they are clearly imagining something that neither they nor any one else has ever seen. If the physicist maintains that the world which we observe is not so solid and massive as we imagine it to be, what he really means is that the world as he imagines it is not so massive and solid as we see it and feel it. He is setting the construction of the scientific imagi-

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nation against the observed facts. He is holding that the real world cannot be observed but only imagined. This other world that science has created for us, is in fact, much more unlike the familiar world which we observe than any of the other worlds that the fantasy of the religious mythologists of earlier ages ever produced. This is one thing that Kant realized about science. The scientist sets out with the conviction that there is something in the world that we don't know. He tries to discover it, and so to bring it within the charmed circle of our experience. Yet instead of doing this, he succeeds only in inventing a new mathematical formula. This formula is an invention. Kant's question is, 'if we invent our scientific knowledge, what right have we to call it knowledge? How is it distinguished from imaginative fiction?'

This observation, however, only concerns the construction of theories for the understanding of the observed facts. Kant saw that the imagination plays a large part not merely in the construction of theory but in the observation of the fact itself. What we call our experience of facts is much more largely the work of our imagination than we ordinarily suppose. At the

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very least it depends to a great extent upon memory.

The significance of this can best be revealed by an illustration. Suppose, then, that when I come into a room I see a blue vase standing on the edge of the table. I go up to the table and stand there looking down at the vase. Then I turn away, and in turning round I brush against the vase and knock it off the table. It falls to the ground and it breaks. Suppose that some one hears the crash and calls out to ask what has happened. I shall reply that in turning round I knocked the blue vase off the table and broke it. Now, let us be philosophical and ask how I know that I knocked the vase off the table and broke it. The answer will surely be that I know it as a matter of direct personal observation. It is a part of my own experience. I did it and I know what I did. What if I am pressed to be very exact and to give a psychological account of that experience? I shall find how small a part direct observation has to do with it. How much of what I express by saying that I knocked the vase off the table and broke it is a matter of direct sensory observation? First I had a visual sensation. I express it by saying, 'I saw a blue vase on the table.' Perhaps it might

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be questioned whether I have any right to say this instead of saying that I had a visual sensation of a blue shape. But we may ignore these refinements. What is certain is that when I turned round the vase disappeared. I no longer saw it on the table. The next direct sensory experience was a slight sensation of pressure on my elbow. That sensation was quite momentary. A second later, I experienced a sensation of sound. That is sense-datum number three. On hearing the sound I turned round quickly and I saw instead of a blue vase on the table some pieces of blue and white china on the floor. At the most, it would seem, I have four separate and discrete sense intimations about what happened. There are two visual sensations, a sensation of pressure, and a sensation of sound. Three of them have no inherent relation of any kind to one another. What is there that is common to a sound, a colour, and a feeling of pressure? Nothing at all; they are just completely and finally different and unrelated. The only thing that they have in common is that they all happened to me. On this slender basis of four different sensations I have managed to construct a connected little story about a vase on the table which I knocked off the table and broke. This

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is a simple example of what we call the direct observation of facts. It is easy to see that the greater part of it is the work of our imagination which pieces together a few fragments of direct sensory experience by filling up the gaps between them so as to produce a story that hangs together. That in principle it is the imagination which does it can be easily seen. If I ask myself what I mean when I say that the vase fell to the floor, when as a matter of fact I did not see it fall, I shall have to answer that I mean that if I had been looking in the right direction midway between the time when I felt the pressure on my elbow and heard the crash, I would have seen the vase midway between the top of the table and the floor. In other words, when I say that the vase fell to the floor, I am imagining what I would have seen if I had been looking, in order to link up my two visual sensations of the vase on the table and the fragments of china on the floor, with a series of imagined sensations. If we then generalize this simple case, we can see that the whole of our direct sensory experience is merely a set of more or less discrete and inherently unrelated fragments, out of which by means of imagination we construct what we call the world of observed facts. In-



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deed, what we usually mean by 'the world' is little more than our imagination of what we would see and hear and feel if we were everywhere at once, seeing and hearing and feeling everything all the time.

If this is true, what can I mean by saying that this elaborate construction for which my imagination is responsible is knowledge? Perhaps the simplest answer would be that in calling it knowledge I mean that any one else would see and hear and feel in the same way that I do under the same conditions; that other people can see what I have to imagine and that, where we all are compelled to pass beyond the limits of observation, there is some necessity which compels us to imagine the same things in the same way.

This answer, however, even if it is satisfactory, is highly abstract and general. The question remains how we are to discover whether the imaginary constructions that we make on any particular occasion are true or false. How are we to distinguish between fact and fiction, if both fact and fiction are the products of the same activity of imaginary construction? So long as we are only worried about theories we can comfort ourselves by saying that they can be tested

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by observation. But when we realize, as Kant did, that the observation itself is largely an imaginary construction and that in its turn it requires to be tested, we have to go deeper. How can we be sure that the element of construction inherent in our activities of observing is itself valid? It is here that we can discover the function of experiment and the importance of experiment in our efforts to reach truth. We can test both our theories and the observation on which they are based by our success or failure in acting upon them. If our imaginary construction cannot be used to anticipate correctly the results of our actions, they cannot be valid. The successful use of our imaginary construction is in fact our final court of appeal. It is not an absolute one, but it does eliminate a large number of claimants to truth. The pragmatist is no doubt wrong in claiming that what works is true, but he may be right if he limits his claim to the statement that what does not work is false. This question, however, is not strictly relevant to our present inquiry. What we have to notice is that experiment is an essential part of our means of distinguishing between constructions which are knowledge and constructions which are merely efforts of the im-

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agination. Above all, we must remember that an appeal to experiment or verification is not an appeal from thought to perception but an appeal from reflection and imagination to action, from theoretical experience to practical experience.

What is important in Kant's analysis of experience is his realization of the extent to which our knowledge, even when it is based upon direct sensory experience, is a product of the activity of the imagination. When we realize this, we realize that the limits of our control of the constructive activities of the mind must be limits for science. This explains why it is a fundamental principle of scientific method to exclude from consideration whatever cannot be verified by observation and experiment. So soon as our minds pass beyond the limits of what can be verified in this way, we have no longer any means of distinguishing between reality and fiction. The imagination can carry its constructive spontaneity far beyond the bounds of possible experience. The difficulty is not to construct systems of belief. That is all too easy. The difficulty is to find any means of distinguishing between different constructions in a way that will guarantee truth. From a psychological point

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of view there is no inner distinction between a construction that is true and a construction that is false. All of them are alike fictions in the strict sense of the term. To distinguish those that are merely fictions from those that constitute knowledge there must be an appeal to something other than the reflective activities of the mind. If ideas are to be knowledge there must be a reference from ideas to reality, and this reference, as Kant has shown, is not to be found in sense-perception alone. The appeal must be from the field of experience in which we are dealing with ideas to the field in which we are dealing with real things, that is to say, the field of action. Where that appeal is impossible, our systems of belief remain speculative. They are not merely hypotheses but hypotheses which it is impossible to test. Scientific knowledge is necessarily confined within the limits of that which can be verified in the field of practical experience through observation and experiment.

This limitation, however, is one which is shared in some sense by all our knowledge whether it is scientific or not. There is another limitation which we must proceed to notice. If we are to observe and understand anything in

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a scientific way, it is essential that our activity of observing and understanding should not alter it. Our purpose is to discover what the object is and how it behaves; it is not to make it different or to interfere with its behaviour. Now, of course, we can attempt to change things and to alter their behaviour. Indeed, all our actions upon the external world are efforts to do so. But if we are seeking knowledge, this is precisely what we must not do. If our observing and experimenting did make a difference to the object, the result could not be a discovery of what is there, or of how it behaves. There is an opposition, as it were, between our theoretical and our practical activities. If we are to know the world we must see to it that it really is an external world. That means not merely that the world must remain external to us but that we must remain external to it. In other words, we must not act upon it. We must remain 'over against it'. Knowledge and action seem to rest on opposite postulates. In knowing anything we demand that our activity should make no difference to it. In acting on anything we assume that our activity does make a difference to it. It is true that unless we know what a thing is we are not likely to act upon it successfully; but on the

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other hand, if we are to discover what it is we must refrain from acting upon it and consider it as it is in itself. We must observe how it behaves when we are not responsible for its behaviour. It follows from this that scientific knowledge is only possible so far as we can stand apart from things and observe what is happening without interfering. Now, to a certain extent, we obviously can. If my bath overflows, I can stand and watch it in order to discover how long it will take to cover the bathroom floor two inches deep. Of course, I can also turn off the tap. But I cannot do both things at once. I must choose between action and discovery. Here we can see, from another angle, why the world of which science gives us knowledge is an external world. The world, of course, is not really external to us, or to put it the other way round, we are not external to the world; we are part of it. But when we seek to know the world in the way that science does, we must withdraw, as it were, from any co-operation in the activities of what we are observing and let things behave in their own way without ourselves being implicated in their behaviour. It seems, therefore, to be a postulate of scientific activity that the object we are seeking to know

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must be independent of our activities of knowing. Perhaps it would be even better to say that the intention which guides our activity must not be allowed to interfere with the behaviour of the object which we are observing. Our attitude to the object must be purely passive. Our activity must be limited to the processes of our own minds.

Now, there is a field of experience where it is impossible to maintain this distinction between knowing something and acting upon it. What is the situation if I want to understand what I myself am doing? Is it possible for me to observe, in the way that science demands, my own actions? If I stop performing an action in order to observe it, there is no longer anything to observe. My actions depend upon me and they only exist because I do them. It is quite clear that in this case I cannot stand aside and observe what is happening as if it were external to me and independent of me. At the best, I shall have to divide myself into two parts, one of which tries to observe the other. I shall have to treat the observed part of my activity as external to, and independent of, the activities of the observing part. Whether this is even partially possible is questionable. It must make

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some difference to the activity that I am observing that another part of me is occupied intently with observing it. And it must make a change in the character and quality of my observing that I have to keep the activity that I am observing going while I observe it. From the point of view of science, as we know it in fields of research where we are trying to know things that are in a real sense independent of us, the situation in self-observation is highly unstable and unsatisfactory. The need which psychology feels for dividing the self into two, and calling one of them the unconscious, is perhaps not unconnected with this dilemma. At least it would simplify the situation if there were a part of our own activities which was unconscious; for then it would stand in the desired relation to the conscious part. It would be independent of the conscious activities and 'external' to them, in the same kind of relation to the observer that holds in other fields of scientific investigation.

Let us put this in a more general and philosophical form. It is a postulate of scientific knowledge that the activities of knowing should not alter the object which is being known. This postulate cannot be granted if the object is myself, for when I try to know myself my activities



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of knowing do necessarily alter me. I cannot first achieve an understanding of myself and then use the understanding I have gained to change myself. Because the activities of understanding are themselves changes in me. The very intention to understand is an intention to change my mind. This is true in every field of scientific research. It only becomes a problem for the scientist when his mind is the object or part of the object which he is seeking to understand. Scientific inquiry seems to demand that there shall be a real object quite independent of the investigator and that it must stand in certain relations to other objects independent of him. It assumes that what happens to the object and how it behaves depend upon its properties and its relations to other objects in accordance with objective laws. This puts the scientist in a dilemma when he undertakes the investigation of human behaviour. He is involved in investigating his own behaviour, but his postulates compel him to consider it as something real which is independent of himself. How can his own behaviour be independent of him? If it can be, what can be meant by calling it his behaviour? And if its investigations are to cover the whole field of human behaviour, will it not be neces-

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sary to make the assumption that there is no part of his behaviour, not even his activity of investigating his behaviour, which is really his behaviour? And since all human beings are in principle observers, at least in the sense that the knowledge which is sought is valid for them all, will not this come near to implying that there is no behaviour which is human behaviour; that there is therefore nothing to investigate and no human activity of investigating it?

This is one of those points at which it is important to remember the warning with which we began. We take it for granted that scientific investigation in the psychological field, as in any other field, neither requires nor permits of justification. The paradoxical statement to which we have been led is not a conclusion; it is merely the statement of a problem. The conclusion that this rules out the possibility of scientific psychology is not admissible. Already in certain fields scientific psychology has provided for itself the same kind of justification as the other sciences. The field in which it has done so is limited, and the extent to which it has done so is still small in comparison with sciences such as physics and chemistry. But in principle it is the same kind of justification. Already psychological sciences

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have thrown a great deal of light upon problems of human behaviour which have until now remained wrapped in mystery, and already the results of psychological investigation have proved themselves capable of successful application. On the other hand, we must not allow ourselves to be blinded by this pragmatic verification to the problem which is presented by the facts. It is astonishing that by treating human behaviour as if it were independent of human beings and external to them we are able, as human beings, to construct by the activities of the imagination systems of theory which enable us to act purposefully and successfully as human beings in our human behaviour. If we face the implications of this paradox instead of trying to explain them away, we may find that it throws considerable light upon the nature of scientific knowledge in general. Though it is only in the psychological sciences that this paradox becomes urgent and pressing for the scientist, it is not absent from the other sciences. It is in principle contained in the equally strange fact that by considering the circumference of a circle to be made up of an infinite number of straight lines, we can reach important conclusions in geometry. The answer that I shall suggest to

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this problem is that science is instrumental knowledge. This answer does set limits to scientific knowledge by implying that there are other kinds of knowledge which are not instrumental and, therefore, not scientific. But the kind of limitation which this suggests is not in the ordinary sense a limitation of the field of fact which is open to successful scientific research. My answer means that the whole field of experience can be scientifically investigated, but that the knowledge that results from a scientific investigation of the whole field, or of any part of a field, is not the whole of the knowledge that can be gained about it. Science can cover the whole range of phenomena but not the whole of the possible knowledge about any of it. It is not limited to a part of the field but to a particular aspect of any part of the field. In other words, there is nothing that science cannot give us some knowledge about, and there is nothing that science can give us complete knowledge about. It should be noticed that this is quite compatible with the possibility that there are parts of the field in which science can give us relatively more knowledge than in others, and that the really important knowledge in one field may be scientific while in another part the

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scientific knowledge may be less important.

We have already recognized that it is proper to seek for a sociology of science. It is legitimate to ask why modern society has produced science under certain definable social conditions and at a certain point in social development. Now, since scientific research is a deliberate human activity, it must have an adequate motive. It cannot simply happen. And since science is a co-operative activity which develops from generation to generation through a continuity of organized effort, the motive which sustains it must be a general and continuous social motive. Part of a scientific sociology of science must be an investigation of the social motivation of scientific investigation. Science is intentional, and therefore no account of it which treats it as independent of intention and, therefore, of human purposes can possibly be considered complete. It is a serious activity which claims and receives loyalty and self-sacrifice from serious and able human beings. Its intention must be serious and urgent. To account for science simply as man's effort to satisfy his curiosity is clearly superficial and inadequate. Such an explanation does not take science seriously. It would need a very curious kind of curiosity to

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produce science, and human beings have been trying to satisfy their curiosity from the beginning without producing science. Scientists, surely, are not trying to satisfy their private curiosity but are following a desire to take part in a human task with ends and effects which pass far beyond their private interests and to which an intelligent man can honourably and serviceably devote his life. The urge which maintains scientific research is surely not to be accounted for by a mere reference to animal instincts. It demands a recognition of rational purpose. To understand science sociologically we must determine the social purpose which is clearly embodied in its development. We must consider the social function of science. We must understand the motive which leads society to sustain the activity of scientists with its approval and respect and even with financial endowments upon occasion at the expense of the taxpayer. Why should it? It cannot be a desire for knowledge. That might explain why certain individuals are chosen as scientific researchers rather than others. It will not explain why society supports science itself. We do not satisfy our desire for knowledge by making it possible for other people to be scientists. The social mo-

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tive which sustains science must be looked for not in the theoretical field but in the field of practical social needs. It is surely obvious that the purpose which expresses itself in the creation and development of science rises and can only arise from the recognition that science enables us to do things that we want to do and which cannot be done without it. Science serves human society by helping it to live the kind of social life that it wishes for, and which it could not achieve without science. For the individual scientist the pure desire for knowledge may be a sufficient motive, though we should not too readily assume that he will be a better scientist for that. For society, however, science cannot be an end in itself. If it were no one would be interested in the progress of science except the small group of peculiar people who wanted scientific knowledge for its own sake, and in that case science would not get very far. Society has its scientists, encourages and supports them, because it needs them for very sound and urgent practical reasons. Beyond a certain level of civilization science becomes one of the necessities of life.

This is to say that from the point of view of society science is a means to an end. It was not

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this, however, that I meant when I defined science as instrumental knowledge. There is a sense in which all knowledge which is intentionally sought is sought for a purpose beyond itself. It is in a more direct and inherent sense that science is instrumental knowledge in a way other kinds of knowledge are not. There are forms of knowledge which we seek which are not scientific and cannot be. When we deliberately set out to increase our knowledge and understanding of beautiful things we are not after science. When we make an effort to understand the people we love and to get to know them better we are not committed to scientific research, and in the whole range of science there is little that could help us. There is a kind of knowledge which is called wisdom. Men have often sacrificed a great deal to achieve it. That kind of knowledge is not scientific. Does any one really think it ever could be? A man may be a great scientist and yet a very foolish fellow—as his wife will surely have occasion to tell him a good many times in the course of his married life. Such simple considerations make it rather obvious that science does not exhaust the field of knowledge but is a special kind of knowledge and, therefore, subject to a limitation. The pur-



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pose which sustains scientific research must likewise be a special purpose, and, therefore, limited. For other purposes we shall need other kinds of knowledge. In calling scientific knowledge instrumental, we are trying to distinguish science from other kinds of knowledge with which it might tend to be confused; and if we can specify the particular purpose which gives rise to this special kind of knowledge we shall in the process discover the limits of science.

To say that science is instrumental means that it is the kind of knowledge we require to enable us to use things. It implies that the social motive which has created and sustained scientific research arises from our need to discover how to use the means at our disposal in the world to the best possible advantage. The motivation of science is ultimately concerned with the provision of technique. This does not necessarily imply that the scientist himself is conscious of the social motive which underlies his work. He is specialized in society to achieve this kind of knowledge. He may have no personal desire to use the knowledge he achieves or even to know whether or how it will be used. For all that, it is quite possible that the only function which this knowledge can perform in human

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life is a technical one, to enable society to use the world more effectively and successfully. To say that science is sustained by the desire for technical power, does not in any sense mean that the desire for technical power is the motive of the scientist. Consider a parallel case. A banker is specialized in society to deal with money. For him the proper handling of money as money may well be the limit of his interest and a sufficient motive for his personal activities as a banker. He may use money merely for the sake of money, dealing in money and looking no further; just as the scientist may pursue knowledge for its own sake and when he has got some may use it merely as a means to get some more. Yet, unless people needed money not for its own sake but as a means of getting other things for their own use, there would be neither money nor bankers. Money cannot be an end in itself in general, though it may be an end in itself for the specialists into whose charge it is entrusted. The individual motive of the banker may be a pure interest in banking; but the social motive for having banks and bankers cannot be. Just so the motive that sustains the scientist in his task may be a pure love of science, though the social motivation of science

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must be different. Men can only live at all by inventing techniques which give them power over their environment. Societies can maintain themselves under reasonably favourable conditions for long periods by the employment of relatively simple traditional techniques. But if they are to progress beyond a limited and impoverished existence it can only be by the deliberate invention of more elaborate techniques, of more complicated machines and methods, in a word, by the invention of the instruments of control. Science is the kind of knowledge of the world that is needed for this purpose, and it is for this reason that we can define it as instrumental knowledge. It is because society intends progress that it needs science, and so long as that intention persists it will be the business of scientists to carry on the pure search for scientific knowledge in the service of society. The ultimate significance of the discoveries they make may lie in the use that is made of them, yet their own interest need not look so far. The scientist may feel that the applications of his science are not his business; though it is difficult to see why he should, since he is a member of society as well as a scientist. But if he does feel so, this will not necessarily

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interfere with his efficiency as a scientist. It might conceivably, under certain circumstances, increase it.

The sort of relation which holds between scientific knowledge and other types of knowledge may perhaps best be seen by means of an example. Let us suppose that by chance a plot of waste land adjoining my own residence has been put up for sale, and that I hasten to acquire it, not for any particular purpose which I have in view but merely to prevent it from being used in a way that would spoil the amenities of my own house. Having bought it, I sit down to decide what to do with it and how to do it. Now let us consider the different kinds of knowledge I shall require if I am to reach a satisfactory decision. We may divide them for our present purpose into two groups, the first containing all the items of knowledge which I acquire in order to determine which of all the possible uses to which the ground might be put is the one which is most worth while, and which will give most satisfaction to myself and any other people who may be involved. The knowledge in this category will be, broadly speaking, knowledge of what I want most. In the second category we shall put all the items of knowledge

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which I shall require in order to decide how the purpose that I have formed can actually be achieved. Roughly speaking these two divisions correspond to the distinction between end and means. In either of them I may show ignorance and make mistakes which will involve practical consequences of an unpleasant kind, and therefore if I am to reach a satisfactory decision it is knowledge in the strict sense that I require. In determining the end, for instance, I may give undue weight to a passing fancy and find when I have accomplished my purpose that it has been wasted labour because I no longer find any satisfaction in the thing that I have produced. Some one will certainly tell me in that case, if I do not tell myself, that I ought to have known better. On the other hand I may also make a mistake in my reckoning of the means required for carrying out my chosen project. I may judge quite rightly what would be best worth doing and yet fail because the project turns out to be impossible to execute, or because it proves more costly than I can afford.

Suppose, then, that I decide to turn it into a flower garden. That will be a satisfactory decision only if my judgement that the ground will be more worth while as a flower garden

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than in any other use is a true judgement. It may be a false one, and where the possibility of truth or falsehood enters we are dealing with knowledge. Suppose that I am right in this judgement. I may still be wrong in my decision for purely technical reasons. I may not know enough about the conditions necessary to produce a satisfactory flower garden, and it may turn out that the condition of the soil or some other factor makes it either impossible or beyond my means to turn my plot of land into the garden I intended. I may achieve my garden and find that as a garden it is a disappointment.

There is another kind of knowledge involved which it is a little difficult to put into either of our two categories, although it has more affinity, perhaps, with the first than with the second. The satisfactoriness of my decision depends partly upon the kind of capacity which I possess, whether by nature or training, to appreciate and enjoy a flower garden. Knowing how to enjoy something is not the same as knowing that I want it more than anything else, though, in many cases, these two kinds of knowledge tend to shade off naturally into one another. A man may be a great lover of music, for

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example, and yet his enjoyment of music may be much more limited than it need be because he does not know how to appreciate it. That this is a matter of knowledge, at least to some extent, is shown by the fact that his appreciation of music can almost always be developed by proper training. That it is not technical knowledge is shown by the fact that the training he requires is not a training in technique, but in musical discrimination and aesthetic judgment.

It is clear that only a limited number of the questions which I should have to answer correctly if I were to reach a satisfactory decision could give rise to scientific research, or be settled by scientific methods. In principle it is the questions in our second category which are amenable to scientific treatment. No one who knows anything about science would imagine that any scientific method could decide for me what is most worth doing or possessing, or which of two pieces of music is aesthetically the more satisfactory. This indeed is what is meant by saying that science is not concerned with values but only with matters of fact. But when I have made up my mind what I want and begin to ask how to set about getting it the questions which then

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arise are just those which can be investigated by scientific methods and which, if they are at all complicated, can only be satisfactorily answered by means of science. It is in this sense that the limits of science are the limits of instrumental knowledge. The kind of questions which science is designed to answer are questions which arise when we are considering the means of doing something, and not the kind of questions which arise when we are considering what is worth doing. Science, in the last analysis, is knowledge of how to use the means at our disposal to achieve our purposes. It is not knowledge of what purposes are good.

I do not wish to give the impression that it is necessary to decide what we want to do before we can set about the business of producing scientific knowledge. A simple analogy may help us to avoid this mistake. Money is a very general means of procuring what we want. This is its only real value. It does not follow that we must decide what we want to buy before we set out to make money. The generality of its use makes this unnecessary. We know that we shall want to buy all sorts of things, and that money, under normal conditions, will buy anything that is for sale. So we can set about providing ourselves



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with money and postpone the question of deciding what we want to buy until after we have saved what we consider enough. In the process the making of money may become an end in itself, and the desire to feel that we could buy anything we wanted may become an even greater source of satisfaction than the actual buying could be. In this way a motive can be generated sufficient to keep us at the business of making money when there is no longer any thought of using it to buy things. It still remains true that, however we feel about it, money is, in fact, merely a means for buying things, and not an end in itself. Now science is even more general in its uses than money, and therefore it is equally easy to set about amassing scientific knowledge while leaving the question of what we shall use it for to be answered later as occasion may arise. It is even easier in the case of science for the process of amassing knowledge to become an absorbing pursuit in itself and to generate a sufficient motive for carrying it on without any thought of putting it to any use at all. We can become knowledge-hoarders even more readily than money-hoarders. There is indeed more than an analogy underlying this comparison. The amassing of knowledge and

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the amassing of capital by modern society are, as we have noticed, interrelated phenomena from the sociological point of view. They are the material and the ideal aspects of the amassing and organizing of power, and have the same psychological root. Power is, in fact, like money or scientific knowledge, which are its two main aspects, inherently means and not end. It is indeed only definable as the means of doing things. It is characteristic of modern society in the West that it has pursued power as an end in itself. Indeed, the psychological root of the present crisis in Western society is that inhibition, to which attention has already been drawn, which prevents us from considering seriously the necessity of changing our own social habits by deliberate intention. To determine socially the uses to which we shall put the power we have amassed, whether as wealth or as scientific knowledge, would be to consider the deliberate organization of a planned society, and would therefore involve an intentional alteration in the traditional form of our society.

The preoccupation with organic phenomena which characterized the second period of modern reflection has had the effect of over-emphasizing what is common to human and animal

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life. The differences are very great, and of profound significance. These differences centre in the intentional character of all characteristically human action. To live intentionally is to live by knowledge, and to live by knowledge does not mean merely to possess knowledge but to use it in the practical field for the determination of action. Now, intentional behaviour depends upon a knowledge of value. When I choose to behave in one way rather than another, when I prefer one course of action to another, I imply that I know that some things are better than others. I may be wrong. I may find out my mistake later and regret it. We see from this that the knowledge of value is a first condition of human action so far as it is characteristically human. The knowledge which is essential to intentional behaviour falls as we have seen into two categories. We need to know what we had better do and we need to know how to do it. This is the basis of the current philosophical distinction between intrinsic and instrumental value. We can value things in two different ways, for themselves or for their use, and both of these ways of valuing can be generalized. We can look upon the world from either of these points of view. If we envisage it from the point

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of view of intrinsic value we look upon it as good in itself and therefore to be enjoyed. If our valuation-attitude is instrumental the world appears as material which can be used. In that case the questions which arise will take the form —‘What use is it?’ There will be produced in this way two different conceptions of the world corresponding to the two valuation-attitudes. We might call them the conceptions of the World-as-Means and of the World-as-End. Of the two it is clear that the conception of the World-as-Means is logically dependent upon the conception of the World-as-End. The end determines the means, not *vice versa*. Intrinsic value is the ground of instrumental value. To decide to use something you must have decided that something else in relation to which you can use it is intrinsically good. Science arises on the basis of the conception of the World-as-Means in the sense that when the instrumental valuation attitude governs our practical thought the questions which arise for reflection to answer refer to the World-as-Means. In other words, the answers, if they are true, can be applied in action in determining how to do what we have decided to do. Scientific knowledge arises out of instrumental valuation and functions in the

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economy of human life in relation to instrumental valuation. Since, however, instrumental value has no significance apart from intrinsic value the significance of scientific knowledge is relative to an intrinsic knowledge which cannot be scientific. An age for which all knowledge is scientific is an age whose valuations are all instrumental, and which is ignorant in the field of intrinsic values. It is an age which has fallen a victim to the irrationality of turning what is means into an end in itself. It knows how to do things without knowing how to choose what to do. It is all dressed up with nowhere to go. It is, in fact, an age of machinery.

To maintain that science is limited to the field of instrumental value, that is to say to that aspect of the world in which it is the means or the instrument in action, still leaves the whole field of fact open to scientific research. It is possible to conceive anything at all as usable for some purpose or other, even if in many cases we are not in a position to use it. It does not follow that this is a satisfactory way of dealing with everything. There is one point at which the effort to deal with things as instruments for our purposes must certainly prove unsatisfactory and in which, therefore, the effort to pro-

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duce the scientific knowledge which could be the basis of control is bound to lead into difficulties, not in the theoretical field necessarily, but certainly in the practical field. This point is reached when the effort is made to establish the psychological sciences. These sciences are concerned with people and their behaviour. Now, it is possible to use people as instruments in the service of purposes. But there is a dilemma involved which we may overlook if we do not remember that this is only possible if other people use them. It is not satisfactory to say that in general human beings exist as instruments of a purpose, and to consider this as a satisfactory account of their nature and behaviour. It would imply the existence of some superhuman agent who used them as instruments, and it would involve the view that no human purposes are real. For if human beings really have purposes they cannot merely be instruments. Even if we admit the curious paradox that a man can use himself as an instrument for his own purposes, that would imply that he must be conceived both as an instrument and as the user of an instrument. There would be an aspect of his existence in which he was not properly conceivable in instrumental terms.

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Now, if science is instrumental knowledge, a science of human behaviour can only be an account of human beings in their instrumental aspect. The knowledge which the psychological sciences achieve, since it is instrumental, has significance only because it enables us to control and use people for our purposes. Only so far as that is possible can the psychological sciences have any significance, and only so far as it is intrinsically desirable can they have any real value. Apart from this, the human behaviour which uses this knowledge will not itself be accounted for by the psychological sciences, for it is the behaviour of human beings acting not as instruments but as using human beings as instruments, acting, that is to say as masters or employers, not as servants employed.

There are, however, practical limits to the possibility of using people as instruments for our purposes. Strictly speaking we can only use people if they consent to be used. We talk too easily about people being compelled to do things that they do not want to do. Strictly speaking, a human being cannot be compelled to do anything. We can only compel a person to do anything by providing him with a motive for doing it. We can make other people serve us, if we can

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make them sufficiently afraid of us. But then they act as they do from their own motive of fear, and the kinds of things we can compel them to do in this way can only be those which do not require their sympathy and support for our purpose. What a man can do from fear is strictly limited. In other words, if we make people fear us, we produce a situation which prevents the existence of any real human relation between us and them. And the purposes which can be achieved through the co-operation we have established on this fear basis, cannot be really human purposes. All the real human purposes require the willing co-operation of all the persons concerned in their achievement. Thus, unless we are prepared to treat other people as having purposes of their own, and, therefore, to treat their activities as possessing an intrinsic and not merely an instrumental value, we shall be compelled in turn to forgo those purposes of our own which are really and characteristically human. The question, 'How can we use people?', if it defines our general attitude to human beings, is at bottom a stupid question. It may have a limited validity provided that relations have been established between all the people concerned upon another



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basis, a basis not of instrumental but of intrinsic value for which knowledge which is not instrumental but intrinsic is the foundation. The psychological sciences must ultimately rest upon the question, 'How can human beings use one another?' But the answer to this question, and therefore the application of scientific knowledge to the instrumental aspect of human relationships will be very different if it is the master who asks 'How can I use my slaves?' or the community of free men who ask 'How can we be useful to one another?'

## Chapter 4

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After the philosophical interpolation in the last chapter we can return to our consideration of the psychological sciences and consider the problem as it arises in psychology itself. We have seen so far that the attempt of sociology to construct a sociology of science reveals a dilemma which suggests that there are limitations to scientific knowledge. Sociology is a part of science, yet part of sociology is the sociology of science. At this point science, in the inevitable course of its development, has been reflected back upon itself and becomes part of its own subject-matter. It is the philosophical implications of this self-reflection of science which we are concerned to examine.

The reflection of science upon itself is in-

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evitable as soon as science extends its field of research to include any general aspect of human behaviour. The reason for this is that science itself is a part of human behaviour. Even economics involves this self-reflection, because the scientific study of economic behaviour is itself a modification of economic behaviour. But it is in psychology that the implications of the reflection of science upon itself show themselves most clearly, because psychology is not concerned with a particular aspect of human behaviour, but with human behaviour as such. It is the general science of human behaviour, and so the focal science of the whole group of sciences which are concerned with any of the aspects of human behaviour. The science of psychology was only satisfactorily established in recent years, only indeed when it was recognized as the science of human behaviour. Previously psychology had been looked upon as the science of the mind. Our discussion of the sociology of science enables us to understand why this prevented the development of a scientific psychology. The inhibition which permitted progress in the theoretical field while preventing it as a deliberate effort in the practical involved the dualism between mind and matter. In conse-

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quence the total field of experience was divided into a subjective and an objective part and scientific knowledge was established as knowledge of the objective in experience. The possibility of attempting to understand the field of subjective experience could not be overlooked, but because this field was defined in distinction from the objective field, which is the external world, the world of objective fact, it was impossible to investigate it scientifically. The mind is precisely that aspect of experience which is not open to observation and experiment, and which is contrasted with the world of objective fact. So long as this inhibition held psychology could only be conceived as the science of the subjective. This, however, is a contradiction in terms. Whatever can be the object of a scientific inquiry is, of course, objective. Its existence must be independent of the activities of the observer. A science of mind could only be possible if the subjective activities of one person were open to the direct observation and investigation of another. If, however, we change our point of view and define psychology as the science of human behaviour the difficulty disappears. Human behaviour is observable; it is open to inspection and experiment like other objective phenomena.

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A scientific psychology becomes conceivable. We see again why the emergence of psychological science depends on the weakening of the inhibition which maintained the separation of theory and practice throughout the first two stages of modern social development. So long as persons are thought of as minds the attention is confined to the theoretical aspects of human activity divorced from their setting in the objective world. What has made scientific psychology possible is a shift of interest from the theoretical to the practical, and therefore material, activities of human beings. And these practical activities can only be conceived as human activities if the hard and fast distinction between subjective and objective has broken down, and the unity of the two has been, in principle, admitted. I have drawn attention in the title of this chapter, to the form which our problem takes in the field of psychology proper. If psychology is the science of human behaviour then there must be a psychology of psychology. For psychology is itself one of the phenomena of human behaviour. Any general theory of human behaviour, if it is to be valid theory, must be applicable to every aspect of human behaviour, and so to the behaviour of the psychologist in producing psychology.

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To ask the psychologist to provide a psychological account of psychology would seem in principle to be a perfectly proper request. To reject it in principle would mean setting limits to the field of scientific inquiry. It would mean that part of the field of human behaviour was admitted by the scientist to be inaccessible to science. Psychologists do attempt to answer analogous questions. I have seen in examination papers set to psychological students questions of the type: 'What psychological conditions would tend to make a man a painter?' If this is a legitimate question it is difficult to see that it could be illegitimate to ask: 'What psychological conditions would tend to make a man a psychologist?' We are familiar with philosophical accounts of the nature of science, and scientists are apt to look askance upon them. As a philosopher I find myself anxious to have, not a philosophical, but a scientific account of science, an account which is based upon strictly scientific investigation. When I ask myself to which department of science I ought to apply for such an account the answer is quite clearly, 'To psychology.' I find indeed that modern psychologists have already attempted to provide scientific accounts of religion, and of art. In the case

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of religion at least some of these scientific theories have been widely accepted. I can see no reason therefore why corresponding theories of science should not be produced. If a psychologist were to provide a scientific account of science based upon strict psychological investigation, it would, of course, apply to psychology as well as to the other sciences. Indeed, the account which the psychologist gives, being a part of science, is part of that of which it is an account, and if its account is to be true, it must be a true account of itself.

It is difficult to see how this can be possible. We noticed earlier that the scientific attitude postulates that the activities of knowing make no difference to the object known. Now we see that in the development of science we must reach a point at which the activity of knowing is itself the object about which scientific knowledge is required. If the postulate is a necessary one it would seem that it must be impossible to have any scientific knowledge of what we are doing when we are behaving as scientists. It would seem that science cannot know itself.

The difficulty arises because at this point the distinction between subjective and objective has broken down completely. In any piece of scien-

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tific research the scientist must adopt the attitude of an observer in relation to the object about which he is seeking knowledge. Even if he makes an experiment the events which he observes in the course of the experiment must not be such that they could not have occurred, in principle, apart from his activity. It must be possible to feel sure that if the conditions which he has arranged in his laboratory had been arranged by any one else, or if indeed they had happened in the course of nature, precisely the same events would be observed. He must not interfere with what is happening. Of course he is doing something all the time. He is observing and thinking and he may make mistakes both in the observing and the thinking. But his activities, by which any scientific knowledge that results is achieved, of which, indeed, science is the product, must have no effect upon the processes which he is observing. In other words the total complex of activity, that of the scientist and his object, must be divided into two sets of processes, a subjective and an objective set, and these two sets of processes must have no causal relations with one another. What happens in the one set must be quite independent of what happens in the other. It is the objective processes



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which are known. The subjective processes are the knowing. It is in this sense that science is knowledge of an objective world. Psychology, however, must include the subjective processes in its object. It may still be possible to maintain the necessary distinction between the particular processes of observation and thought through which the psychologist produces his scientific theory of other processes of human behaviour involving observation and thought. He may even observe and think about the behaviour of other psychologists in producing psychological theories, or, in retrospect, he may observe and think about his own activities as a psychologist at other times. It has even been thought that a kind of introspection may be possible by which a psychologist can observe and think about his own subjective processes while they are actually being carried on. This is very doubtful, and even if in a sense it were possible it would involve a division of attention which must necessarily modify the character of the processes themselves. But at the best it is clear that in psychology the distinction between subjective and objective aspects has become purely relative. In the other sciences the intentional activities of the scientist are totally excluded

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from the objective field. In psychology any actual process involved in the activity of knowing may fall either on the objective side or on the subjective. The distinction has ceased to be an objective distinction and become itself subjective, variable and dependent upon the observer.

This, however, is only one aspect of the problem, and not the most important. The distinction between subjective and objective processes is not a purely formal one. It cannot be made at any point which happens to suit our convenience. It implies a difference in the nature of the processes themselves. From the scientific point of view, to say that a certain process is objective does not merely mean that it happens at the moment to be the object we are considering. It means also that it is matter of fact, an integral part of a system of occurrences which happen in accordance with objective laws, and not as the expression of a personal intention. To make any particular occurrence the object of scientific research is to assume that it happens determinately in accordance with necessary laws which are themselves objective and independent of any personal will or intention. All this is implied in saying that for science the

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objective world is the world of fact. In psychology then to consider human behaviour objectively is to consider it as matter of fact, as something that happens in accordance with determinate laws which are independent of human will and intention. If the same process is considered both as subjective and as objective from different points of view it is considered sometimes as the expression of a personal intention, and sometimes as an occurrence which is independent of any personal intention. The scientific knowledge of it must take the form of the discovery of a law which determines its occurrence. In other words any scientific psychology of science must represent science as something that happens in accordance with universal laws and not as something done by human beings in order to achieve a purpose. Such an account of science must be, to say the least of it, rather odd.

To consider anything as matter of fact and as happening in accordance with an objective law excludes the possibility of its being right or wrong. Fact is simply what is there or what happens. In the objective world there can be no mistakes. To imagine that there could would be to think that the laws determining what happens could be broken. It would involve adopt-

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ing an anthropomorphic attitude, and thinking that the events we were observing were the acts of some one seeking to realize a purpose and capable, upon occasion, of failing to achieve his purpose. Nothing can happen rightly or wrongly. If we judge that anything is right or wrong, it must be with reference to some intention which is, or is not, realized. An action can be done rightly or wrongly because it is informed by an intention which it may or may not realize. But an event cannot happen rightly or wrongly. To consider it as an objective event is precisely to exclude the reference to intention, in terms of which it might be judged right or wrong. Since a scientific psychology must regard human behaviour objectively, as determined by necessary laws, it must represent human behaviour as unintentional and any judgement which implies that it is good or bad behaviour, proper or improper, satisfactory or unsatisfactory, right or wrong, is precluded.

Now, when we are dealing with any part of human behaviour which is theoretical in its aim the intention which informs it is the intention to know something. Scientific activity is undertaken in order to achieve scientific knowledge. In terms of this intention the results of the

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activity, that is to say, the conclusions which are reached, may be true or false. If they are true they have realized the intention of the activity which produced them. If they are false they have not. In either case they can be true or false only because they are determined by the intention to achieve knowledge, and only with reference to this intention has the description of them as true or false any meaning. But if the psychologist gives a scientific account of his own behaviour in producing psychology he must consider his behaviour as a set of processes which happen in accordance with objective laws. This means, however, that the idea of their being true or false is excluded. By making them the object of scientific research he has made it impossible to regard them as activities informed by an intention in terms of which they might be correct or incorrect. From the scientific point of view a scientific theory is simply a belief held by a certain number of people. A scientific account of it would show that these people necessarily held these beliefs and could not, under the conditions obtaining, hold any others. In showing this, it would also show why certain other people necessarily refuse to accept these beliefs and could not do anything but re-

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ject them; just as a scientific account which shows that a stone must fall to the ground if it is free to do so will show also that a balloon must rise from the ground if it is free to do so. The question whether the beliefs are true or false cannot arise. Thus we reach the paradoxical conclusion that the psychologist's account of psychology, if it is to be scientific, must exclude the possibility of considering it either true or false. His theory must be a theory such that, if it is true, it cannot be true.

We should not be justified in accepting such a conclusion merely on the strength of an abstract argument. It must at least be corroborated by an empirical reference to the actual results of the effort to establish psychological theory. But first we may notice that its significance becomes clearer when we translate it from a purely theoretical discussion into practical terms. Let us assume that we are correct in defining science as instrumental knowledge. This means, as we have already discovered, that the scientist's objective world is not the world in all its aspects, but only in its aspect as a means or instrument. The theoretical distinction between subjective and objective elements in our total experience corresponds from the practical point of view to

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the distinction between that which is used in achieving a purpose and the agent who uses it. That which is used, the instrument, is passive to the agent. The purpose is the agent's purpose, not the purpose of the instrument. The instrument being merely the means for realizing the purpose can have no purpose of its own. If it had it would be agent and not instrument. On the other hand, it can only be the instrument of an agent's purpose if it has a determinate character of its own which is independent of the agent's purpose in using it. And it is by knowing what its character and properties are that the agent can use it in the accomplishment of his purpose. This knowledge of the character and properties of the instrument which are independent of the intention for which the instrument is used, is instrumental knowledge. Now if we seek for a scientific knowledge of human behaviour, we are viewing it as part of the 'World-as-Means'. We are looking upon human behaviour as having a determinate character in virtue of which human beings can be used for a purpose. In that case their behaviour must be considered as unintentional. The intention could only belong to the agent who used them. Now, it is possible, under certain conditions, to

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use human beings as instruments for a purpose. The simplest condition is that they should consent to be used, and agree to hold their own purposes in abeyance and remain passive to the purposes of another person. It is possible, therefore, to set out to discover what can be done with people who consent to be used as instruments. It is possible to think of systems of human belief in this purely instrumental way. I need only recognize the fact that a group of people hold certain convictions and ask how people with such beliefs can be used. Indeed, propaganda is based upon such a consideration. If I can persuade people to believe that life is not worth living without a motor-car, then I can use their belief as a means of increasing the production and sale of motor-cars. But from this point of view the truth or falsity of their belief is irrelevant. All I need to know is how they can be brought to believe it, and how the belief can be maintained. If now I universalize this attitude I can ask how human beliefs in general are produced. Whatever conclusion I come to, however, will itself be a human belief which, if it is true, has been produced in the way it asserts that all beliefs are produced. My belief about the origin of human beliefs must itself be con-



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sidered instrumental, and from this point of view it can neither be true nor false, but merely the result of the operation of certain objective causes. On the other hand, this view of beliefs has now become meaningless, because all agents are now looked upon not as agents but as instruments. No purpose remains in virtue of which any belief could be true or false. In other words, from the scientific point of view, all beliefs, including scientific beliefs, occur to people. The processes which cause them to occur are unintentional, and therefore, the beliefs are not the realization of a human intention to achieve knowledge. If the belief that all beliefs are brought about in this way is true, then, since it is a belief, it cannot be true. For to say that a belief is the product of the operation of objective forces which necessitate its occurrence under certain conditions, is clearly incompatible with holding that it is believed because it is true. It is, in fact, not the kind of thing of which truth or falsehood can be predicated. If the belief that I reach is psychologically necessitated and is the product of conditions under which it is formed, how can I raise the question whether it is true? If I did, and came to the conclusion that it was, my belief that it was true would in

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turn be psychologically necessitated.

It has been held, in some quarters, that recent discoveries in biology have demonstrated that our beliefs are bound up with the activities of the endocrine glands. Suppose for a moment that this theory is true. What is the psychologist who accepts it to do? If he writes a book to prove it, he invites the retort that both his beliefs and the reasons he gives for them merely express the conditions of his own endocrine system, and that since mine works differently I cannot possibly be convinced by anything he will say. He is surely wasting his time. He should rather be perfecting his techniques for modifying the working of my physiological system in such a way that it would produce the same beliefs as his. On the other hand I cannot see why, when the techniques were available, I should not use them on him to bring his beliefs into line with mine. The question, 'Whose beliefs are true' has been resolved into the question, 'Which of us is physiologically the healthier', and that is not a question that can be settled by argument. This is no doubt a rather crude example, but it has the merit of showing rather clearly the sense in which it is possible to frame a theory such that if it is true, it cannot be true.

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It should be noticed that this conclusion only follows on the assumption that science covers the whole field of knowledge in all its aspects. It is only if the psychological theory is applicable to every aspect of human behaviour without exception that it produces such a paradoxical result. If there are limits to scientific knowledge then there may be other forms of knowledge, which are not scientific, in terms of which the truth of scientific theory and the limits of its validity can be known. There may be an aspect of all human behaviour which is validly expressed in a general psychological theory, provided there is another aspect which can be known in some other way. It is only if this is denied, and all knowledge is considered to be scientific in character, that we should reach the conclusion that if it were true it could not be true, because it precluded the consideration of truth and falsehood, and reduced all beliefs to the status of mere fact. For the moment, however, we are concerned to consider psychology as if it were a complete account of the nature of human behaviour as a whole. From this point of view we can employ the reflection of science upon itself to provide a negative criterion by which to judge the validity

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of psychological theory. If psychology gives us an account of human behaviour which cannot possibly be an account of the behaviour of the psychologist in giving it, then the theory cannot be accepted as a true account of all human behaviour. We can ask about any form of psychology, 'Supposing it to be true, does it explain the human behaviour that produces it and judges it to be true?'

Let us apply this test, in the first instance, to the form of psychological theory known as behaviourism. Behaviourism is a good example to choose, for two reasons. The difficulty which we are considering, though not peculiar to behaviourism, exhibits itself in clearer and more comprehensible fashion in behaviourism than in most other theories. Secondly, behaviouristic theory is the result of a determined effort to stand firmly by the principles of scientific method, and to deal only with what can be objectively observed and what is susceptible of experimental verification. These two things, it seems to me, are not unconnected. The more strictly the psychologist holds to his scientific principles of method, the more clearly the difficulty presents itself. The behaviourist tells us that all human behaviour is reaction to stimulus,

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that therefore it is possible to understand any human action in terms of general laws which determine that this particular action is necessitated by that particular stimulus from the environment to which it is the response. Since it is clear that the same stimulus does not produce the same reaction throughout the life of a particular individual; since, in fact, human beings are clearly capable of learning from experience, the processes by which this modification of reaction is brought about present an immediate problem. The behaviourist accounts for this process of modification by what he calls the conditioning of original simple and instinctive reactions to determinate stimuli. Thus, every form of developed human behaviour is said to be a 'conditioned reflex', that is to say, a determinate response to a stimulus the form of which is dictated by two factors, the original instinctive response and the conditions in the past experience of the individual which have modified the response to that particular stimulus. How are we to discover whether this view is true or not? It might be thought that it could be done by experiment. This, however, would be a mistake. For the purpose of the experiment we have to postulate that the be-

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haviour which is observed is in principle a reaction to a stimulus. What is in question is the interpretation of the experiment. Instead, we may ask what account can be given, on this theory, of the theory itself. The theory has been produced by human beings and is being criticized and defended. Its production, its assertion, the attacks upon it, and its defence, are all examples of human behaviour. The theory maintains that all human behaviour is a reaction to a stimulus. If this is true, then the production of it as well as the activity of attacking or defending it are reactions to stimuli. The belief in it or the disbelief in it are also reactions to stimuli. What then can be meant by asking whether the theory is true? The question whether it is true can only be another reaction to a stimulus which has to be explained as a conditioned reflex. On the other hand, it is hardly open to doubt that when the psychologist stated his theory he meant it to be true, and not merely his reaction to a stimulus. He does not think that in offering this account of human behaviour he is merely producing another sample of behaviour to be accounted for in terms of the kind of conditioning which his original instinctive reactions to the environment

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have undergone in the course of his life. If he thought this, what he would affirm would be that he necessarily believed that all human behaviour was a reaction to stimulus because of the particular kind of life he had led. It would follow that he believed that any one who disbelieved the theory did so because his past experience necessitated the disbelief. What he could not reasonably mean is that he is right in believing the theory and his opponent is wrong. If the theory is true, what explanation can be offered of his intention to produce a true theory? Neither intention nor truth can be explained; they must be explained away. Nothing that a man asserts can be asserted as the truth. It can only be the result of influences which have played upon him since infancy. Thus we find ourselves with the paradox upon our hands which we have already noticed. If the theory is true, whatever that can mean on this view, it cannot be true; since the idea of truth is ruled out by the theory itself. Moreover, since the theory is merely a conditioned reaction to stimuli, it cannot have any intention, and so it cannot mean anything.

I wish to repeat at this point that this paradox is not in itself a reason for denying the

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possibility of a scientific psychology. It does not even warrant us in saying that behaviourism is not a valid scientific theory of human behaviour. It was to avoid such a misconception of the meaning of the argument that I insisted in the beginning that it is not the business of philosophy to justify or condemn science, and that science neither requires nor permits of justification. The paradox merely states a problem about the nature of scientific knowledge as a whole which reveals itself within science when it reaches the stage of laying the foundations of psychology. The difficulty arises not because the subject-matter is psychological but because the method is scientific. If we feel tempted to conclude, as philosophers, that the argument shows that science can tell us nothing about the human mind, we should remind ourselves that a modern physicist, like Professor Eddington, has been tempted to conclude that science can tell us nothing about the nature of the material world. In both cases, the conclusion arises from the recognition of the same difficulty, that the distinction between subjective and objective elements in experience is, in the last analysis, a subjective distinction. For my part, I feel unable to accept the conclusion in either case. It seems



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undeniable that science tells us a great deal both about the physical world and about ourselves. The conclusion, however, is not inevitable. It may arise through a misconception of the nature of scientific knowledge, and a revision of our assumptions on this point might remove the difficulty. If we proceed to consider this possibility, then the problem which arises through the reflection of science upon itself may reveal something important, not merely about the nature of science, but also about the nature of human knowledge and human life in general.

To follow up this clue, we must seek to understand why the psychologist gets into this paradoxical position. How does the behaviourist hypothesis arise, and what is the difficulty that it is seeking to overcome? It arises through the effort to maintain the objectivity that scientific method demands. The object of which science gives an account must be independent of the processes in the observer of which the account is the product. It arises from the determination to deal only with facts which are empirically observable and empirically verifiable. Unless we can do this our results cannot claim to be scientific. They will be infected with subjectivity. If there is to be a science of human behaviour,

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then human behaviour must be matter of fact, observable and verifiable. The behaviourist, therefore, insists on observing human behaviour and allowing the observed facts to suggest a hypothesis which shall explain them, and demands that it should be the kind of hypothesis which can be verified by further observation. This attitude forces us to make a distinction between the observer and what he observes. He observes something in the external world behaving in a certain way. The way in which it behaves must not be affected by his observing. In the case of psychology this can only mean that one person, the psychologist, observes the behaviour of other people. He is the observer. All other persons are potentially objects for him and his observation and experiment. His behaviour in observing them is subjective and does not come into the picture. Their behaviour, on the other hand, is objective and is unaffected by his observation. It follows from this that he is assuming, perhaps unconsciously, that at least a certain aspect of his own behaviour is different in kind from the behaviour of the people he observes. There is a part of the sum total of human behaviour which is not observed, and which is indeed excluded from consideration by

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the demands of scientific method. It cannot form part of the observed facts upon which any scientific hypothesis must be built. It is excluded because it differs in kind from the behaviour which is observed. Consequently, its exclusion from the range of fact that is admissible as the basis of a hypothesis is the exclusion, not of certain particular facts of the same kind as those which are observed, but of a different kind of fact about human behaviour than any upon which the scientific hypothesis is built. The psychologist's behaviour in observing and thinking and imagining and constructing hypotheses falls outside the fact which he is seeking to understand. The strict conditions of scientific inquiry demand that it should do so. The very activity of observing makes a distinction between the behaviour of the observer and the behaviour of what he observes. It is, therefore, not difficult to understand that the explanation given of the behaviour which is observed should not be a satisfactory explanation of the behaviour of the observer. It was never intended to be.

This demand, that the observed behaviour should be objective fact in the sense that science requires, limits the field of psychological inves-

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tigation more severely than might at first be supposed. In practice, the demand means that the behaviour observed should not be the behaviour of an observer. Now, all human beings are potentially observers, and if the psychologist is to be strictly scientific in his observation he must secure conditions such that the behaviour of the persons he observes is not entangled in their activities of observing. If the results of the inquiry are to have the required universality, they must be such that any observer whatever could, in principle, be the observer who is responsible for them. It is also important that the person observed should not be conscious that he is under observation, since it is difficult to ensure that this will not modify his behaviour. In experimenting, the psychologist must see to it that his subjects behave 'naturally', that is to say, as if they didn't observe that they were being observed. This in itself is apt to introduce an artificiality into the behaviour that is under observation. It is only in very simple forms of behaviour that it is possible to achieve this kind of artificial naturalness. All the most characteristic forms of our behaviour are, indeed, conditioned and modified by the fact that we perform them in the presence of other

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persons and with the consciousness that we are being observed and judged. But even in the simpler forms of psychological experiment, if the subjects insist on taking an active part in the investigation, instead of doing precisely as they are told, they are apt to defeat the purpose of the investigation. For satisfactory scientific results, the two kinds of behaviour must be isolated as it were, in different persons. The one carries on the subjective activities of observing and theorizing, the other carries on the types of behaviour which it is desired to investigate and account for. It may be possible for one person to observe his own behaviour, but if he does so, it is no longer quite 'natural', and it is not easy to be sure that the effort to do both things at once does not modify both. The scientific demand is that the observing should make no difference to the behaviour observed, and this means at least that the behaviour of observing is not itself part of the behaviour which is observed.

From this we must conclude that the psychologist must impose conditions on human behaviour in order to secure the kind of behaviour that can be objectively observed and accounted for. He obviously cannot take the view that he

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is the only observer and that his behaviour is immune from observation. The effort to understand human behaviour is an effort that every human being can make. The observer can always be observed. He is compelled, therefore, to distinguish by implication between two kinds of human behaviour, which might be called, in default of a better term, natural and reflective. The natural behaviour is so much observable fact. The reflective behaviour is natural behaviour modified by the agent's consciousness of it and the intentionality to which that consciousness can give rise. The distinction is in practice very difficult, if not impossible, to draw. It is, in fact, the distinction which is involved when we try to decide whether some one's action was intentional or accidental. Suppose that an important document in my possession has been destroyed. In clearing my desk I have dropped it along with other papers which are of no value into the waste-paper basket and it has been burned. Under normal circumstances it would be assumed that this was quite accidental and my friends would commiserate with me upon my loss. But if it was subsequently discovered that I stood to gain a large sum of money by the destruction of the document a doubt would

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arise. People would find themselves wondering whether I had not purposely dropped the document into the waste-paper basket to produce the appearance of an accident. How is this difference between intentional and accidental behaviour to be determined? If I intended the destruction of the document and have set about the fraud with sufficient cleverness I shall have produced a situation in which there is no observable difference between the two cases. I may have seen to it that there were observers present when the 'accident' occurred. They may have seen me clearing my desk, examining the papers, and dropping them in the waste-paper basket. Their observation will be unable to detect any difference between an accidental and an intentional consigning of the document to the flames. In this case, there is no way of deciding whether it was an accident or not, except by what we call circumstantial evidence. The reason why circumstantial evidence is always rightly suspect is that it involves an inference which cannot itself be verified by observation. Any attempt to prove intention from observation of fact must be logically invalid. Where we feel that it can produce a moral certainty that the act was intentional, this certainty rests upon our own

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direct experience of intentional action, and not on observation. The very fact that I can deceive other people and deceive them intentionally proves that there is an element in human behaviour which is not observable. It is, indeed, this element in human behaviour which makes it characteristically human. It is the determination or modification of the observable, objective behaviour by subjective intentions. It is, in fact, what is referred to in human behaviour by the term 'reason'. We must conclude, therefore, that what is observable in human behaviour is not characteristically human. It lacks the element of intention, which distinguishes the purposeful action of a human being from the mere happening of events in the external world. Indeed, we might say that whatever is observable is by definition an event, whereas what is not observable is an action; since the very thing that distinguishes an event from an action, the fact that an action is a piece of behaviour that is intended, is not observable. Seen from the outside, objectively, a human action appears as an event happening. If science is to maintain the strict objectivity which its method demands, it must treat all observed changes as events. A psychology which is to carry science into the



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field of human behaviour must, therefore, treat human actions not as actions but as events which happen in the external world. It must seek for their explanation in their interrelation with other events which are equally objective and in laws to which such events conform. It must not refer them to subjective activities such as conscious purposes. As observed events they are mere matters of fact without any suggestion of intention behind them. They are accepted as observed facts. In this way, they cannot be described as right or wrong, since that distinction refers to human acts not as events but as the realization of intention. If then the human behaviour which is to be accounted for is the kind of behaviour which produces a psychological theory, and if it is treated as matter of fact, as in principle a series of objective events, then the activity of the psychologist in his work cannot be performed rightly or wrongly, and the theory which it produces cannot be true or false. For that distinction refers to the intention to achieve knowledge which the psychologist's behaviour either realizes or fails to realize.

Let us now take another example from a different type of psychological theory. Since we are primarily considering the psychology of

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science, it will be well to choose an example which offers an analogy to such a question for our consideration. Science is a characteristically human type of activity. Religion is another; and in certain fields they are so analogous that they can come into competition by offering incompatible accounts of the world. Now modern psychology has already provided us with various scientific theories of religion in general. Of those we had better choose the one which has been put forward by Professor Freud on the basis of psycho-analytic investigation. I choose the Freudian account of religion not merely because it is familiar but especially because, of all the schools which employ the methods which Freud was the first to devise, his own school is distinguished by the strictness of its effort to remain true to the fundamental principles of scientific method. The Freudian theory of religion maintains that it is an illusory form of human behaviour, which provides through the operation of fantasy imaginary satisfaction for repressed tendencies in the human *psyche*. The beliefs to which this activity of fantasy gives rise are not true beliefs. Their function, indeed, is not to satisfy a desire for knowledge but to provide a substitute in imagination for the satisfac-

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tion of impulses which cannot be satisfied objectively and which are accordingly suppressed from consciousness. The energy system of the child's *psyche* finds its original outlet in its relation to the father. The wishes which are generated in this relation, and which have a natural field of satisfaction in it, persist after the situation itself has been destroyed by the growth of the child to maturity. These early desires can no longer find real satisfaction. They are repressed from the intentional consciousness of the adult because this is recognized. They remain active, however, in the unconscious and provide themselves with imaginary satisfactions by projecting the father-image upon the world, and inventing forms of behaviour which appear to consciousness as referring to the existence of a supernatural father who stands in the same relation to the adult as the earthly father does to the child. In this world of illusion the childish wishes which persist in the unconscious find a substitute gratification which is denied them in reality. In this way, the primitive natural impulses of the *psyche*, when they are denied their natural satisfaction, build up in consciousness a world of symbols to which the unconscious alone possesses the key. When, by the scientific

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methods of observation and analysis the key is discovered, and the symbolism interpreted, we discover that the symbolism provides an imaginary satisfaction of primitive wishes which have no longer any relation to the real world of adult life. As soon as their meaning is recognized, they are seen to be illusory.

This is not, of course, intended to be an adequate account of the Freudian theory of religion and it includes no reference to the mass of data on which the theory itself is founded. But we are not concerned at all with the question of whether it is a good theory or not. We are concerned only to notice the general character of the account which it seeks to give. It is an account which in form must be applicable to any of the types of reflective activity which characterize human behaviour. Art, for example, demands the same kind of explanation, and indeed Freud himself and other members of his school have sought to provide a similar theory of art. Suppose then that we take the principles of Freudian theory as the basis of an attempt to provide a psycho-analytic theory of science, and incidentally of scientific psychology. The account must necessarily follow the same lines and conform to the same type. Science is not a primary or

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natural expression of the activity of the *psyche* any more than art or religion. It involves the repression of the primary wishes and the reflection of psychic activity into the construction of symbolic ideas in consciousness. The construction of scientific theories is the work of fantasy, and the original wishes which are gratified by the activity of fantasy in the work of the scientist must equally be unconscious desires seeking and obtaining their gratification in a symbolic form. Science will be the expression of the repression of primitive wishes into the unconscious and, therefore, an imaginary wish-fulfilment. If this type of explanation leads properly to the conclusion that religious beliefs are illusory, it is difficult to see how we can avoid the conclusion that science also is an illusion. But a scientific theory which maintained that science is an illusion would be holding that it was itself illusory.

It might be thought that it is a sufficient reply to point out that scientific theories can be verified, while religious beliefs cannot. This, however, is not so. Religious belief is open to the same kind of verification as psychological theory, and religious people are continually pointing to verifications in experience of their religious beliefs. Indeed, precisely the kind of

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cures of neuroses which are the main verification of psycho-analytic theory are to be found in many forms of religion as the result of the acceptance of religious beliefs which are considered by most people nowadays, including religious people, to be illusory. The psychologist may reply that the verifications produced by religious people are as illusory as the beliefs that they verify. Yet this is precisely the kind of argument that the scientific psycho-analysts have had to meet from their opponents. They have pointed to the cures they have effected, as evidence of the truth of their theory, and their opponents have replied that the verification is illusory. Even if it is admitted that the patient now behaves more normally, that only proves that what he now believes produces results in his behaviour which are more satisfactory to himself and to other people. This does not prove that what he now believes is true, and certainly it does not prove that what the psycho-analyst believes is true. It is always possible to find illusory verifications for illusory beliefs. One is reminded of the schoolboy who was asked whether there was a pole at the North Pole. 'Yes,' he replied, 'an imaginary pole.' 'Could you hang a hat on it?' asked the teacher.

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'Yes,' the boy answered, 'an imaginary hat.'

We must again remind ourselves that this does not prove that the Freudian theory is not a valid theory. We are not concerned to justify or to disprove it. That is only possible for the scientific psychologist. We are merely formulating the problem that it offers, when we push its conclusions to the limit. Indeed, I see no reason to deny that all the reflective activities of the human mind, including science and philosophy, are, in fact, the products of fantasy, and have their original motive force in the repression of primitive childish wishes. This may well be true. My contention is only that it is not possible in principle to select some of the constructions of reflective imagination and dogmatically to declare them exempt from the kind of account that is offered of the rest. Science, art, and religion, considered from the purely objective standpoint of science as matter of fact produced by the activity of persons, stand on the same footing. If one of them is illusory because of the manner of its origination, then all of them must be. This is a problem which we have to consider, not a conclusion which we have reached. It is plainly ridiculous as a conclusion, since, if all beliefs are illusory, then no belief can be either

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valid or invalid. If all beliefs are illusory, then the belief that all beliefs are illusory must be itself illusory. There is no longer any meaning to be attached to the distinction between true and false. Consequently, it is quite useless to ask questions and try to answer them, because no answer could be of any more value than another. The conclusion arises because the attempt to achieve a science of human behaviour must treat human behaviour as observable fact. It cannot, therefore, deal with any aspect of human behaviour which involves, not matter of fact, but matter of intention. An activity which began by being intentional may become habitual. It may, that is to say, continue to be performed after the intention which originally established it has disappeared from consciousness. The difference between these two ways in which the same activity may be performed is not matter of fact and is not observable. A scientific theory must ignore the distinction between them, if it is not to lose its objectivity. Yet all the deliberate activities of human reflection, such as religion and art and science, are illusory, so far as they are unintentional. It is an illusion to imagine that a talking parrot speaks. It merely produces sounds similar to those which are produced by



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human beings when they speak. To say that reflective activities are unintentional is simply to say that the reason for carrying them on is not present to consciousness, either because it never was present or because it has disappeared from consciousness. A scientific theory of these activities must, therefore, ignore as unobservable the intention which may or may not be present. It must treat them as mere matter of fact. We can now see that to do this is to *assume* that they are illusory. This curious result, which expresses itself in the paradoxes which we have discovered, is accounted for by the nature and limitations of scientific knowledge. That it is only a philosophical problem is proved by the fact that the scientific psychologist must treat human behaviour in this way because of his own deliberate intention to be strictly scientific. It is because science is an intentional activity of a determinate kind that science must treat itself as unintentional if it is to give a scientific account of itself. Our knowledge of science as an intentional human activity, which enables us to value science, to produce it and to use it, is not itself scientific. Thus we are brought back to a point which we considered earlier, that there are forms of knowledge which are not scientific.

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We have found another reason for recognizing this. If science were the only form of real knowledge, then science itself would be an illusion and not knowledge at all.

When we discussed the sociology of science we found ourselves seeking for an explanation, in terms of psychological conditions, of the appearance and development of science as a special form of human behaviour. Now, the production of scientific psychology is the final stage of this effort, and it is possible to attempt to account for the emergence and development of psychology in the same way. The psychological conditions which underlie the production of science will underlie the production of psychology, but we shall have to add to these an account of the specific conditions which determine that the science produced shall be psychology rather than any of the earlier sciences. The science of psychology attempts to discover by scientific methods the conditions which determine human behaviour. Since producing psychology is itself a form of human behaviour, the sociology of psychology will be an attempt to discover the determining conditions of the production of psychological science. There must be a motive—not necessarily conscious—underlying any

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characteristic form of human activity. There must, therefore, be a motive for the recent development of interested effort in the psychological field. Part of this motive will be the general motive which determines the development of science. In addition, there must be a special reason why scientific psychology has not developed until quite recent times and which will account for the direction of so much human interest and energy in our own time towards this special activity. We must ask ourselves now how we can account in psychological terms for this direction of effort.

One might be inclined to say that people have always been interested in themselves and wished to understand the working of their own minds. That it is only recently that a scientific understanding has been attempted might, then, be explained by pointing out that only recently has science reached the stage of development at which the attempt became possible. This is no doubt true, but it is not a satisfactory answer to the question. It does not tell us why people have always been interested in the working of their own minds; even if they have, which is open to doubt. But, in particular, it does not explain why people have come to want a scientific account

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of the working of their minds. On the whole, it was the people who have always been interested in understanding the workings of their own minds who have been the most strenuous opponents of the development of scientific psychology. Religion and philosophy are the expressions, to a large extent, of a universal interest in the attempt to account for the kind of things that psychology deals with, and it is certainly not true that the motives which underlie the development of modern psychology are to be found most strongly at work amongst religiously or philosophically minded people. The desire to know about our own minds must itself be of a special kind if it is to determine the development of a psychology of a scientific kind. It must be the kind of motive that gives rise to science rather than to philosophy or religion.

We have already noticed that the motive underlying science is bound up with the desire for progress. People who live in a society which sets a high value on the maintenance of tradition and authority in the field of belief, will never produce science. The desire to achieve better forms of social life must condition the desire for better forms of understanding. Conversely, the desire for the development of knowledge is the

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reflective aspect of the desire for better forms of life. Now, the presence of such a desire presupposes that we are at least relatively dissatisfied with our present form of life. Behind the effort to produce physical science lies the feeling that our control of material conditions is not sufficiently good to enable us to live a better life than we do. In the same way, the effort to develop a scientific psychology must rest on the feeling that our control of human behaviour is not good enough to enable us to improve our ways of living. The motive for the effort to produce psychology could not be present unless we were dissatisfied with ourselves.

The statement that we are dissatisfied with ourselves, however, is ambiguous. If I say we are dissatisfied with ourselves, I may mean either that I am dissatisfied with myself and you with yourself, or I may mean that I am dissatisfied with you and you with me. Either of these states of mind may give rise to a desire for effective knowledge of human behaviour. But the effort to achieve it will be very different in the two cases. The state of mind in which I feel the need to understand my own behaviour is very different psychologically from the state of mind in which I feel the need to understand the

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behaviour of other people. This is not unconnected with the motive which compels the psychologist to distinguish between the observer and the observed to which I have already drawn attention. We can understand the difference best by reference to the practical basis of the desire for knowledge, which is the need for control. The need to understand my own behaviour will arise from the desire to control and alter my own behaviour. But the need to understand other people's behaviour will arise from the desire to control and alter *their* activities. In the first case, I am dissatisfied with myself and want to discover how to control myself. In the other case, I may be quite satisfied with myself and have no desire to change my own ways of behaviour at all. Indeed, the most direct way in which the desire to control other people's behaviour can arise is when I recognize that unless I can control the behaviour of other people, I shall be compelled to change my own behaviour against my will.

Now, it is the state of mind in which I desire knowledge of behaviour in order to control the behaviour of other people which corresponds most directly with the character of scientific psychology. The psychologist stands over against

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the persons whose behaviour he observes and seeks to account for. He is the observer and they are under observation. The corresponding practical attitude is the one in which I am the agent and other people are the instruments through which I achieve my purposes. To do this I have to control their behaviour. The state of mind in which I am dissatisfied with my own conduct does not in itself give rise to scientific, but to religious reflection. It is, indeed, the state of mind which we call repentance. The distinction between observer and observed which determines the form of scientific psychology is thus the counterpart of the practical relation between controller and controlled, between governor and governed, or between master and servant. This suggests that the psychological condition for the production of scientific psychology is to be found in a state of mind in which individuals are conscious that their own lives are being frustrated and their purposes rendered ineffective by the activities of other people over whom they have no control or too little control. Such a state of mind can give rise to an effort to acquire the kind of knowledge which will enable the individual to control other people's conduct. If this is so, then the development of

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scientific psychology, as a co-operative social effort is itself evidence that in our own time this state of mind has become fairly general. There must be a widespread feeling in modern society that our lives are unsatisfactory and that the way other people behave is responsible for it. It is this tendency to find the cause of our dissatisfaction in other people rather than in ourselves that is the important point. If we blamed ourselves rather than other people the result would not be the effort to achieve scientific psychology but rather a religious movement on a large scale, or at least something to which religious movements are the closest analogy with which we are familiar.

If we accept this sociology of psychology we can understand why the effort to produce a scientific theory of human behaviour results in paradox when it is pushed to its logical conclusion. We noticed that in any psychological experiment the person whose behaviour is under observation must behave 'naturally'. He must behave, that is to say, as if he were not being observed. He must take no part in the activity of observing because that will modify his behaviour. Scientific experiment has often been described as controlled observation. If the



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method of experiment is to be introduced into the field of human behaviour this can only mean that human behaviour must be observed under controlled conditions, and this must imply that the human behaviour which is observed is itself under control. If I am to control some one else's behaviour, even for the purpose of scientific observation, the other person must be what I want him to be, and not have his behaviour determined by intentions of his own. Either he must be unconscious that I am controlling him, or he must consent to it. If he consents, he subordinates his will to mine and takes orders from me. He becomes in either case an instrument for accomplishing my intentions. Now, there are no doubt conditions under which this sort of relation between one person and another is proper and justified, as for example when a patient puts himself in the hands of a physician. There are also conditions under which it is clearly improper and immoral. But for our present purpose the important point about it is that it cannot be universalized. All human behaviour cannot be controlled for the simple reason that there would be then no behaviour which was the controlling of it. Thus in the corresponding form of reflective activity, which is the effort to

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establish a scientific or instrumental knowledge of human behaviour, the attempt to universalize the theory must result in paradox. It cannot apply to the behaviour which consists in producing the theory. To produce a scientific theory of human behaviour, which will be applicable to all human behaviour without exception, we must assume that all human behaviour is objectively determined, or in other words, that it can be accounted for without reference to the will of the human beings whose behaviour it is. This would imply that our behaviour in producing the theory was independent of our own will and intention, which is absurd. The paradox reflects the irrationality of a social situation in which everybody wants to achieve control of everybody else. Where everybody is trying to control every one else, nobody can control anyone. The intention, if universalized, would become self-defeating. It is this practical irrationality which expresses itself as an antinomy in the theoretical field in the attempt to establish a scientific psychology.

Let us return, in concluding the chapter, to the sociology of science, to discover how this enables us to deepen our understanding of the social process of the production of science. At

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the end of the Middle Ages the desire for progress became socially effective. The first step in the effort to progress was the attempt to acquire the kind of knowledge of the material environment which would allow society to control its environment. That effort was found to be insufficient because it could not be made effective in practice except through the control of social organization. Only a co-operative, organic society could use a scientific knowledge of the material world for the purpose of social progress. In the effort to organize society for progress there emerged ultimately the recognition that the real obstacle to the achievement of the progressive intention lay in the psychological field, in the state of mind of the human individuals who make up society. Unless people could control themselves they could not achieve their intention to organize society for progress. In this way there has arisen the consciousness of a need to discover how human behaviour can be controlled, and science has entered the field of human behaviour. The psychological sciences are seeking to provide that kind of knowledge of human behaviour which could be the basis for the control of human behaviour. This attempt to complete science has, however, landed

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us in a quandary. To control his environment man must control himself, and to control himself he must know how he is made and how, in fact, he does behave. But if he knows how he behaves, how can that help him to behave differently? If he can behave differently, the psychology which enables him to do so cannot be a true and complete account of how he behaves. If he cannot behave differently, what use is his psychology to him, and how indeed could he behave so differently as to produce it? So far from solving the problem with which science began, 'How can we control the world?', its completion in psychology merely sets the question, 'Who is to control whom?', and introduces a universal struggle to control one another which, if it develops, must make all effort to control the environment impossible and make the work of science itself equally impossible. Perhaps it is this which is dimly felt and expressed in those movements of our own times which attack the desire for progress as a mistake and which insist that the true destiny of man lies in the continuous and unending struggle for power.

## Chapter 5

### THEORY AND PRACTICE IN PSYCHO-THERAPY

We have noticed how important a part has been played in the social development of science by the separation of theory and practice. We have seen also that the production of psychological science depends upon overcoming the psychological resistance to their unification. But the form of the psychological sciences, as we have so far considered it, remains purely theoretical, and the control which is exercised by means of psychological knowledge remains separated from the process of producing it. The unification of theory and practice is, as it were, achieved in theory only, through the search for an understanding of the motivation of human behaviour. Consequently, the application of such knowledge remains socially unplanned and

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uncontrolled. It is applied, for example, for purposes of political or economic propaganda, to control the reactions of individuals to advertisements for the purpose of increasing consumption, or breaking down sales-resistance. It is used to develop efficiency of production in industry. In other words, the application of scientific knowledge remains in this field as in other fields psychologically distinct from its production. There is, however, a point at which this separation is itself overcome in the development of psychology, and at which theory and practice are brought into close relation and condition one with another intimately. This point is reached with the appearance of psycho-analysis, which is at once a scientific theory of motivation and a technique of control; in which, indeed, the theory is consciously developed as a means of psychological control. It is, therefore, important to carry our philosophical study of psychology further and to examine the theory and practice of psycho-analysis in the light of the tentative conclusions which we have already reached.

Before doing this it may be well to take stock of our position. We may sum it up in the following way. When science reaches the stage in its

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development at which it seeks to provide scientific accounts of human behaviour, it is faced with the task of giving an account of the human behaviour which produces science. At that point it becomes apparent that science is not something sacred and divine, sitting in judgement upon the world, but only one amongst a large number of human activities, requiring a special motive to maintain it. That motive turns out to be the desire, which is rooted in an increasing social necessity, to achieve control of the environment. In the long run, the control of the material environment turns out to be impossible apart from a controlled social organization; and, in turn, a controlled social organization is found to depend upon social self-control. It is at this point that the problem of the psychology of psychology makes its appearance. The attempt to provide a psychological account of psychology leads us into antinomies. It leads to theories of human behaviour which, if they were true, would make the production of scientific psychology impossible. The effect of the fundamental postulate of science in the psychological field is to divide human behaviour into two contrasted types, the subjective behaviour of the observer-scientist and

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the objective non-scientific behaviour that he observes. The psychologist's theories of human behaviour are accounts of the behaviour which he observes, and for that reason they cannot be accounts of the activity which he carries on in observing and theorizing. The underlying cause of this dilemma we have found in the social motivation which produces science. The motive underlying the effort to produce a scientific psychology is the desire, which has become a social necessity for human beings, to exercise a rational control of human behaviour. That desire, however, can only become an effective purpose if we can settle the question 'Who is to control whom?' This question suggests the division of human beings into two classes, those who control and those who are controlled; and who is going to settle this question? We find ourselves, in fact, presented with the old problem of human freedom in a new dress.

We must not permit ourselves to evade the issue by concluding that a scientific psychology is impossible. A scientific psychology has already been brought into existence. Even if we took the view that scientific psychology is not valid it would still remain true that it had been produced, that it had become the basis of successful action



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and had been verified in the only way that any scientific theory can be verified. Neither will it do to say that the difficulty is only a metaphysical one, produced by the speculation of a philosopher. The problem itself appears within the field of science. The philosopher is only drawing attention to it and trying to understand what it means. His explanation may be merely metaphysical, but the problem is not. We have suggested that the clue to the solution lies in recognizing that scientific knowledge is not the whole of knowledge even in principle, but a limited and dependent kind of knowledge; that it is, in fact, instrumental knowledge, or knowledge of the World-as-Means. This view involves the belief that there is an essential relation between knowledge and action, and that our understanding of the activities which are theoretical must rest upon an understanding of the relation between theoretical activities and practical activities. For this reason, the conscious and intimate relation of theoretical and practical activities in the development of scientific psycho-therapy is of special importance for our immediate purpose.

We may notice in the first place that all psycho-analytic theory rests upon the assump-

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tion that there are two kinds of human behaviour. This is the significance of the belief in the Unconscious. The distinction between the behaviour of the observer and the behaviour which he observes has been universalized, as it were, and reflected back into the individual self. When I am observing the behaviour of another person, the motives and intentions which lie behind it are not open to inspection. In my own behaviour I am aware of its subjective aspect as well as its objective aspect. But in the case of another person I am only directly aware of the objective side. Indeed, so far as observation goes, I can only assume that there is a subjective side from analogy with my experience of my own behaviour. If we follow the customary usage which uses the term 'consciousness' to cover the subjective aspect of behaviour, then we shall have to say that, so far as we are objectively aware, another person's behaviour is unconscious. Or, to put it subjectively, there is no conscious relation between my consciousness and his. Now, if we start from this distinction between my consciousness of my own behaviour and my consciousness of another person's behaviour, we may easily generalize it by saying simply that there are two forms of

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human behaviour, and proceed to locate both in one and the same person. I shall then find myself thinking that there are in myself two kinds of consciousness, of only one of which I can be directly aware, while the other remains a matter of inference. It is quite natural to call this other 'consciousness' my unconscious. It has often been pointed out that the notion of an unconscious consciousness is absurd. The objection, however, is too merely verbal to carry much conviction. It is more important to see how it arises. It is a way of saying that there are aspects or elements of my behaviour which have the same kind of relation to my consciousness that the behaviour of other persons has. I cannot be conscious of its subjective aspect, and yet I am under the necessity of assuming that it has a subjective aspect. What I call the consciousness of another person is simply the thoughts, impulses, desires, and intentions which I must assume to be operative behind the behaviour that I observe. No doubt, I am convinced that *he* is aware of them, but *I* am not, and in the nature of things can never be. The 'unconscious' of the psycho-analyst involves the hypothesis that what is always true of my consciousness of another person's consciousness

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is true also in respect of a part of my own consciousness. It merely insists that I can observe in my own behaviour as well as in that of other people elements of activity which can only be accounted for on the assumption that they have a motive, and to which I can assign no motive. If this is so, then I must have motives which I am not aware of. A motive of mine which I am not aware of is quite properly described as an unconscious motive, and the collection or system of such motives in myself is not too badly described by the phrase 'my unconscious'.

This account of the theoretical origin of the notion of the unconscious in psycho-analytic theory does not, of course, justify the analogy on which it is based. It can in fact be dangerously misleading, particularly if consciousness is objectified and conceived as an entity. This is a mistake, however, which has its origins in the tradition of European philosophy and not in the peculiarities of psycho-analytic theory. The proper distinction seems to be, originally, that between acting consciously and acting unconsciously, rather than between *a* consciousness and *an* unconscious. There is, however, a practical motive underlying the adoption of the hypothesis of the unconscious which con-

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cerns our purpose more nearly. The practice of psycho-therapy involves a relation between doctor and patient in which the doctor has to diagnose a disease which has its seat in the subjective processes which lie behind the behaviour of the patient. The problem for doctor and patient alike is to understand why the patient behaves in a way which is unsatisfactory to himself. In other words, both doctor and patient are under the practical necessity of trying to understand the motivation of the patient's behaviour. Now if the doctor is to help the patient to discover the motivation of his behaviour it is necessary that the patient should adopt the same attitude to his behaviour as the doctor does. The doctor of course can never have that direct awareness of the patient's motives that the patient has when he is conscious of them. He has to achieve his understanding of the patient's mental processes from outside. He is compelled to adopt an objective attitude and to arrive at his conclusions by inference from what he can observe and what the patient can tell him. The patient therefore must adopt the same attitude to his own behaviour. He must consider it as if it were the behaviour of another person, as it is, in fact, for the doctor.

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Now, if the doctor's understanding of the patient's behaviour and its motivation is to form the basis of a scientific theory of the motivation of human behaviour in general this attitude which is practically necessary for the treatment and diagnosis of the illness of a particular patient must be generalized. All human behaviour, including one's own, must be looked upon as if it were the behaviour of other persons, and the direct awareness which each person has of his own inner life must be discounted. No other procedure is possible if a theory which is to be universally valid is to be produced, once it has been recognized that a man's own account of his motives may be mistaken, and that in some cases, at least, he is, on his own admission, ignorant of his own motives. The subjective processes in human behaviour are then abstracted from the consciousness of them, because this consciousness cannot be a general one. This does not deny that each particular human being has a direct awareness of his own subjective processes. It merely recognizes that it is private to the individual, limited, and liable to error. The general theory may confirm it or give reason for mistrusting it. But it has this paradoxical result, that a general theory of the sub-

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jective processes in human behaviour must exclude some of the subjective processes from consideration.

The theoretical antinomy which this involves need not delay us further. It is merely another expression of the paradox that we have already considered at length. But because of the interrelation of theory and practice in psycho-therapy it produces here a contradiction between theory and practice of an interesting and instructive kind. If we take an objective view of our own subjective activities they appear to have the same relation to our consciousness that the behaviour of other persons has. This means, at least, that they appear not to be under any conscious control of ours, but to determine our behaviour independently of any conscious intention of our own. The effort to achieve complete generality in the scientific theory of motivation will thus lead to the view that the whole of our behaviour is motivated objectively, including the processes by which we form our intentions and will our actions in conformity with our intentions. It follows logically from this that what we call our consciousness is controlled by the unconscious, and that our consciousness of our motives and intentions merely

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supervenes upon the objective and unconscious activities which wholly determine its content. In this way the scientific character of any psycho-analytic theory necessitates the conclusion that consciousness is always and at every point controlled by the unconscious. On the other hand, if we turn to the practice of the psycho-analyst we find that its purpose is to bring the unconscious under the control of consciousness. The patient's illness consists in the fact that his conscious purposes are systematically thwarted by features of his behaviour which he is unable to control and to subordinate to his conscious intentions. The effort to cure him is the effort to get rid of the unconscious motives which produce these abnormalities in his behaviour. If the psycho-analyst intends to cure his patient he necessarily assumes that a consciousness which is unable to control its behaviour and which is unable to subordinate its motives to the achievement of its chosen ends is abnormal. If he cures his patient he has succeeded in helping him to bring his behaviour under conscious control. There is here a patent practical dilemma. The practice of psycho-analysis aims at bringing the unconscious under the control of consciousness.



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The theory postulates that consciousness is always under the control of the unconscious. If the theory is true the practice must be impossible. If the practice is possible, then the theory cannot be true. Yet it is impossible to choose either of the alternatives, since without the theory the practice could not be carried out, and without the practice the theory could never have arisen.

We must consider more carefully this distinction between the conscious and the unconscious which lies at the basis of psychological theory, in order to discover what it corresponds to in our ordinary non-scientific experience and manner of speech. If we talk about the conscious and the unconscious without carefully considering what it is that these terms refer to, we are apt to think of them as two entities, almost as two separate persons whose wills may come into conflict and set them struggling with one another. This tendency is not confined to modern psychology. We are familiar throughout the history of philosophy with analyses of this kind. In Stoic theory, Reason and the Passions appear as a higher self and a lower self struggling for the control of behaviour. This is clearly unsatisfactory. We set out to understand

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the unity of the self and the manner of its functioning. We end by believing, or at least talking as if we believed, that the self is really two selves which do not get on very well together. The problem is not solved in this way. It is only complicated. In the same way Plato, in the *Republic*, divided the self into three parts and drew attention to ways in which the different parts could act in opposition to one another. The same metaphor which describes the elements within the self as if they were separate selves, sometimes working together as a team, and sometimes working at cross purposes, lurks in Plato's account and finds a clear expression in his playful comparison of a man to the combination of a man, a lion and a many-headed monster under one skin. This is not meant to be taken seriously, of course, but it remains true that the unity of the self tends to disappear under such analysis and to be replaced by a complex of opposing entities whose relation to one another eludes us. In Freudian theory a similar three-fold division of the self into the Id, the Ego, and the Super-ego—a sub-self, a self, and a super-self—appears, and these three components tend to be talked of as if they were a trinity of personages each with its own distinct

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purposes and motives and ideas. This metaphorical way of describing the complex unity of the self has its uses, but it is obviously dangerous if we forget its metaphorical character. In that case it becomes a sort of scientific mythology. The only way to avoid this danger is to have a clear idea of the direct and immediate experience we have of the facts to which it ultimately refers.

The main fact with which we are concerned is that one and the same person behaves both consciously and unconsciously. Sometimes we know what we are doing, and sometimes we find ourselves doing things without knowing why; even without having noticed that we were doing them. Our normal way of describing this difference is to use the distinction between unintentional and intentional activity. When we behave intentionally the purpose of our action is present to our minds and guides our activity. When we act unintentionally, this is not so. We may do things as a matter of habit, and in that case the action has a purpose, but it is not necessary for us to be conscious of the purpose, because the requisite behaviour in such a situation has already been established as a more or less automatic response to such situations. We

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find also automatic activities which are certainly purposive—such as the way in which we close our eyes at a bright flash of light—which do not seem to be under conscious control, or to have been established in the way that habits are formed. The main distinction is clear. When we behave unconsciously, we are not conscious of the reason for our behaviour. When our behaviour is conscious, we are. Let us take one or two examples which may serve to draw attention to the distinction as we experience it, rather than as we formulate it in analytical reflection. In considering these examples we should remember that we are trying to get behind any kind of analysis and simply to recall certain typical cases in which the difference makes itself felt in our ordinary non-reflective experience. Suppose that I were to draw two parallel lines a foot apart and five yards long on the floor, and to ask you to walk across the room between the lines. You would do this without hesitation and without any concentration of attention upon the process of walking. But if I were to ask you to walk over a plank one foot wide placed across a deep gully five yards wide, the situation would be different. You might be unable to bring yourself to do it at all. If you did,

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you would walk with a concentration of attention upon the process of walking which was entirely absent when you walked between the chalk lines. The act of walking is much more 'conscious' in the one case than in the other. The same kind of distinction can be realized if we think of the difference in our mental states when we walk across a room in the light and in the dark. This example serves to call attention to the fact that the distinction refers rather to the inner than the outer conditions of our behaviour. When we walk in the dark our walking is more conscious than when we walk in the light, precisely because we are deprived of the use of the sense of sight.

Take another example. Two people who habitually share a common life have their habits of relation to one another so fully developed that they normally behave to one another, or in one another's presence, without being 'conscious' of one another. What this means is simply that their intentions are not directed upon one another but upon the things that they are doing together. If they have a quarrel, however, the situation is altered. Their sudden hostility makes them very conscious of one another's presence, and whatever they do will have a conscious

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reference to one another, and this bringing of the relation into consciousness will last until the quarrel is over and forgotten. These examples are interesting precisely because the distinction between consciousness and unconsciousness which is involved in them all is so ordinary and normal, that they seem at first sight to be far removed from the kind of distinction we have in mind when we talk of the conscious and the unconscious in psychology. But the reason for this is that in psycho-analytic theory we are preoccupied with cases of an extreme and abnormal kind which stand, as it were, at the very opposite end of the scale. A general theory of human behaviour must be capable of embracing the whole range of the distinction to which it draws attention. And it is therefore important for us to bring such normal instances of the distinction between conscious and unconscious behaviour into relation with the more extreme and abnormal cases.

The next point which deserves notice is that most, if not all, of our unconscious behaviour is the result of the formation of habits. Modern psychology is apt to overlook this through its very wide use of the term 'instinct'. Strictly speaking, there are extremely few examples of

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instinctive behaviour in human life. Perhaps the instinctive activity of sucking in the human infant is the only one which meets all the requirements of a proper definition of the term. An instinct is a *specific* adaptation of behaviour to the environment which does not require to be learned. Nearly all human activities do require to be learned, and the use of the term 'instinct' in respect of those which are universally learned very early in life is apt to lead to confusion. Habits once formed function in the economy of human life in the same way that instincts do in animal life. The difference lies in the fact that they require the co-operation of consciousness and of intentional activity for their original formation. This is important because it allows for the possibility of their being eradicated or modified by conscious intention.

Another point of importance is that the distinction between conscious and unconscious behaviour is not normally a distinction between different actions but rather between different elements in one and the same action. In any conscious and intentional act there is necessarily a great deal that is unconscious. What is explicitly present to consciousness is merely the goal which is intended. Most of the behaviour

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by which the goal is reached is not itself consciously intended but is automatically produced by the systems of habit which have already been built up. If I wish to cross the street, I concentrate my attention upon the other side and upon the traffic. The movements which I make come automatically in answer to the relevant selection that my consciousness makes in reference to the purpose. If I wish to describe what I am looking at to some one who cannot see it I fix my attention on the object and speak. The words come of themselves. If, however, I were trying to describe it in a foreign language which I knew imperfectly, this would not happen, at least to anything like the same extent. Part of my intention would have to be consciously pre-occupied with remembering the words and putting them together according to the rules. It is only that aspect of the behaviour upon which attention is focused that is strictly conscious. And such conscious activity is only possible on a large substructure of habit which is unconscious activity. Thus, in ordinary intentional behaviour, there is a close interrelation of conscious and unconscious elements which together make up the unity of the action. We are never conscious of the very complex habits of co-



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ordination between our various muscles and nerves which is involved in even the simplest conscious activity.

The distinction between conscious and unconscious activity, in the forms in which we have so far considered it, differs widely from the form of the distinction which occupies the attention of the psycho-analyst. The reason for this lies in the preoccupation of the medical psychologist with a special and abnormal condition. In our ordinary conscious activity the elements which are unconscious, habitual, or automatic can normally be brought into the focus of consciousness whenever the necessity arises. If I find myself constantly slicing my drive on the golf course, I can set to work to discover what I am doing wrong. When I do this I swing the club with my consciousness occupied with the movements of my body. I may soon notice that I am dropping my shoulder, instead of keeping it up. I shall then practice my swing with the conscious intention of not allowing my shoulder to drop, and continue to do this until I am satisfied that the correct habit has been re-established. I can then forget about my swing again and concentrate on the game. What I have done is to bring an established habit which

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is not working satisfactorily from the unconscious into consciousness, where I can re-educate the habit and then allow it to become once more an automatic or unconscious element in conscious activity. The economy of consciousness demands that any element in action which can be satisfactorily performed unconsciously should not be brought into consciousness. It is only, therefore, when the unconscious mechanisms upon which we depend for achieving our conscious purposes fail us and so frustrate our intentions that we have to bring them into the focus of consciousness. The special problem with which psycho-therapy is concerned arises when it is in this way necessary to bring an unconscious element in our behaviour into consciousness, and we find that it is impossible or extremely difficult for us to do so. We find cases in which something in ourselves opposes and brings to nothing all our efforts to bring the unsatisfactory unconscious element to the focus of attention. It is in such cases that we recognize that our only hope of regaining control of our behaviour is to seek the help of some one else who can enable us to do so. Psycho-analysis, we must always remember, grows out of the practical situation which brings a patient to a doctor for

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help. Its theory is the result of the effort of the doctor to diagnose and deal with the patient's trouble. And since the heart of the problem is to enable the patient to bring into consciousness elements in the motivation of his conduct which he cannot himself recover from the unconscious, the practical task of the psycho-analyst is to devise a technique which will enable him to do so. This, however, involves a pre-occupation with such abnormal situations in human behaviour which has important results in the theoretical field. What the doctor needs is an instrumental theory of the unconscious, that is to say, a theory which will enable him to operate upon the unconscious elements in the patient's behaviour. An instrumental theory, as we have seen reason to believe, means a scientific theory, so that we may conclude that what is needed by the psycho-therapist as the basis of his therapeutic practice is a scientific theory of the unconscious. But the production of the scientific theory is only possible through generalization from observed instances, and generalization is always subject to one danger which cannot be avoided. It is the danger of over-generalization. A generalization from observed instances must be stated in universal terms. Yet, it may, in fact,

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prove to have empirical limits, which can only be discovered by the observation of cases which do not conform to the universal rule that has been formulated. The simple empirical generalization that all swans are white is found to have empirical limits by the discovery of black swans in Australia. Indeed, the development of any science consists, in one of its important aspects, in the search for the empirical limits of its current generalizations. In the development of psycho-analytic theory this leads to generalizations about the motivation of human behaviour which are based upon instances in which the motivation remains unconscious in the patient. This tends properly to the universal hypothesis that all human behaviour is unconsciously determined. As we have seen, such a generalization in the psychological field necessarily results in antinomies. It must at least be untrue in the case of the behaviour which produces and uses the theory consciously. But the point which interests us at the moment is that it involves the effort to explain the cases in which conscious and unconscious behaviour work harmoniously together—the normal cases—in terms of the abnormal cases in which the unconscious elements become independent of conscious con-

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trol. We have already seen that this procedure is not the expression of a natural perversity in the psycho-analyst. It is necessitated by the conditions of scientific procedure, and it is a scientific theory that is needed. Nevertheless, the procedure is paradoxical from a common-sense point of view, which would demand that the abnormal cases should be interpreted in terms of the normal; the relatively less known in terms of the relatively better known cases. I propose, therefore, to suggest a simple-minded, common-sense account of this kind; but in doing so I must insist that such an account cannot result in a scientific theory, and, therefore, cannot be in any sense a substitute for a proper psycho-analytic theory. The antinomies to which a scientific psychology gives rise are not evidence that the psychological theory is invalid. They are evidence that the type of validity that is demanded of scientific theory is a special type of validity, which depends upon the kind of knowledge that science achieves.

We can start from the recognition that a conscious activity is not an activity in which every element is conscious. It is an activity in which all the unconscious components are satisfactorily related to the conscious component. The

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conscious component determines the end which is achieved by the activity. The satisfactoriness of the relation consists in the subordination of the unconscious components as means to the achievement of the conscious intention. These unconscious components are mainly habits; and habits are automatic mechanisms in human behaviour which have originally been learned. Many of our habits, including all the fundamental ones, are learned very early in life. Even so, we must bear in mind that learning is always an activity which involves a conscious component. It is not the same thing as the conditioning of reflexes; that is to say, the unconscious modification of original automatisms. It implies that the unconscious, habitual elements in human behaviour were once conscious activities. This, of course, does not mean that they do not in turn contain unconscious elements, like the co-ordination of muscles and nerves, which never were and never can be conscious elements in behaviour. But the co-ordination of muscles and nerves in human behaviour only takes place in a process of activity which has a conscious component. It is only when the process of learning is complete that these activities become wholly unconscious. We are taught to walk and

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to speak when we are very young, and learning to speak or to walk is a conscious and intentional activity. But when we have learned to speak or to walk these activities become unconscious, and they become unconscious because they are now used automatically as means to conscious activities of a more complex order which they make possible. This suggests that the human unconscious is, at least mainly, the product of human consciousness. Psycho-analytic theory, because of its scientific character, tends to the generalization that human consciousness is the product of the human unconscious, but this is merely another instance of the antinomy which is the necessary result of the scientific point of view. It need not, therefore, disturb us unduly.

We have to go farther before the real problem with which the psycho-therapist is faced can come to light. Some of the activities which are carried on in childhood as conscious activities, and which in this way drop from consciousness and become automatic, prove to be incompatible with the development of the intentional life of the individual. They cannot be integrated in the conscious activity of the adult as unconscious means to the achievement of his conscious intentions. What is commonly described as the

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process of sublimation seems to refer to the effort to discover ways in which such unconscious elements can be used as mechanisms for the achievement of mature intentions of a kind which the adult life can consciously accept. It might, indeed, be used in a wider sense than is current, to describe the whole process by which the achievement of early intentions can be used as an unconscious mechanism for achieving intentions of a higher order, which is one of the most characteristic features of human development. It is, however, normally restricted to cases in which difficulty is experienced in doing this and especially to cases in which it can only be done by forming intentions to which such mechanisms can be subordinated, and which are acceptable to the individual or to the society of which he forms part. But there are cases in which this is impossible, either because there is, in fact, no intention which an adult mind could approve, to which they could serve as means, or because the individual, or the society to which he belongs, refuses to accept any such intention. In that case, such formed habits must be excluded altogether from any share in the determination of conscious behaviour; since the only way in which any automatism can



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form part of conscious activity is by being integrated with an intentional component. We are all quite familiar with situations in which we can only behave satisfactorily by resisting the automatic reaction which the situation tends to call out. Whenever we are 'minding our *p*'s and *q*'s' this is, in fact, what we are doing. But in the cases with which we are now concerned, this resistance to the tendency of a habit to determine behaviour has to be general in all our behaviour. We have to create a habit of resistance to this particular automatism. It is not merely repressed in a particular case because our intention in the particular situation demands its repression. It is repressed altogether. There are no situations in which it is allowed to form part of the motivation of conscious behaviour.

It is sometimes possible to deal with this situation by allowing some of these automatisms to function in determining reflective activity only. So far as the life of reflection can be carried on for its own sake, or as an end in itself, this is possible. Elements of habit which are not allowed to form part of the motivation of practical life may still be allowed to function ideally in the conscious creation of ideal constructions

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of one kind or another. Some of these may be accepted by the individual and his society; and they can then be published, discussed, criticized and appreciated. It is also possible for the individual to use them in the creation of private fantasies which he would not reveal to other people. This is all the easier if the ideal constructions are symbolic, in which case it is possible to develop a world of symbolism in consciousness without referring the symbolism to that which it symbolizes, and so without understanding its significance. These mechanisms for establishing habits of repressing, from the determination of overt behaviour, automatisms which are not compatible with the conscious practical life of the individual are necessary because there is a limit to the possibility of complete repression. The more energy there is expended in repressing such automatisms the less energy is available for positive, practical action; and, in the long run, the necessities of practical life determine a minimum of energy which must be expended positively and so a maximum that is available for purposes of repression. It is easy to see, therefore, how situations may arise in which the energy required by an individual to prevent these repressed automatisms from determining

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his practical behaviour may not be sufficient. In that case he will find that his behaviour is escaping from conscious control, that he is doing things that he does not intend to do and which he yet cannot prevent himself from doing. But his habits of repression make it impossible for him to bring into consciousness the elements in the unconscious which are disturbing and frustrating his intentional life. These elements are not integrated with his normal habits. They have remained isolated, as it were, and therefore they could only be brought into consciousness by reinstating in consciousness the original intentions which were involved in their formation. The trouble is that the adult consciousness rejects these intentions, and any expression they can have in consciousness must be in a symbolic form to which the clue is absent.

Now, whenever an unconscious mechanism begins to frustrate our conscious activity it must be dealt with. But it can only be dealt with by bringing it into consciousness and reforming it. In the cases we are considering this is impossible for the individual directly affected. Before their urgency compels him to attempt to deal with them consciously he has developed a technique which allows for their presence by dissociating

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them from his normal conscious behaviour. The very resistances which he has developed in order to limit their expression in his normal activities now prevents him from recalling them to consciousness. He must seek the help of an expert in order to do so. What the expert needs is clearly a technique for overcoming a resistance in the patient's mind which prevents him recalling to consciousness what was once present to consciousness, in most cases very early in life. The general term applied to this incapacity to recall past experience to consciousness is the term 'amnesia'. Psycho-analytic technique is primarily a technique for overcoming amnesia and facilitating memory. If this is achieved, then the repressed element can be brought back into consciousness and the process of re-education of the habits involved can begin.

From this point of view there is no special mystery about the success of the psycho-analyst in curing his patient by bringing the unconscious motives into consciousness. I do not mean that the process of securing the disappearance of the neurosis is an easy one, or that it is easy to understand it in detail; but merely that in principle it is easy to see why, if the process of overcoming the amnesia is successful, recovery becomes pos-

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sible. When any element of behaviour is brought into consciousness it becomes intentional. Indeed, to bring it into consciousness is simply to discover what we are doing when we perform it. If it is in consciousness its performance is necessarily intentional, that is to say, we know what we are doing when we do it. We must carefully guard against the tendency to talk about unconscious intentions. To say that an activity is unconscious, is, in fact, to say that it is unintentional. An unconscious intention is, therefore, a contradiction in terms. It is easy to slip into this mistake because it is impossible to be conscious of an activity without recognizing it as embodying a possible intention. If I think an unconscious activity, of course I bring it into my own consciousness, and it is then difficult to avoid thinking of some one performing it consciously, and so becoming aware of the intention that would need to be present if it were consciously performed. But to be conscious of an unconscious activity would be clearly impossible, if the activity in question were my own present behaviour. But if it is a hypothetical activity or some one else's activity, or an earlier activity of my own, I can be conscious that it is or was performed unconsciously, or unintention-

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tionally. On the other hand, I can only do this by thinking of it as an activity which could, at least theoretically, be performed consciously. I can only think of it as having, as it were, a possible intention. In this way it is easy to slip into the habit of talking about an unconscious intention, when what we really mean is the intention which would be involved in the activity if it were to be consciously performed. By definition, a conscious activity is intentional and an unconscious activity is non-intentional. Intention is the *differentia* of conscious behaviour.

Now, a repressed automatism which is frustrating the conscious intentions of a human being is simply an activity which cannot be a component in his conscious behaviour. If he can bring it to consciousness it becomes at once an activity which can only be performed intentionally. It is, however, an activity which he cannot perform intentionally. If it were not, it would not be repressed and would not be disturbing his normal behaviour. When such an activity is fully brought to the centre of consciousness it cannot be performed either intentionally or unintentionally, either consciously or unconsciously. It cannot be done unintentionally because it is now in consciousness. It cannot

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be done intentionally because the intention which would be necessary is one which consciousness rejects. It necessarily ceases to be a component in the individuals behaviour altogether. This, of course, in itself does not explain the cure, since it is only while the habit in question is in consciousness that it cannot determine behaviour. As soon as it is dropped from consciousness it can again become active as an unconscious determinant. The cure is only permanent if the automatism itself can be broken up and reshaped so as to produce a modified habit which can be integrated with conscious behaviour. For this reason the second stage in psycho-therapeutic treatment takes the form of a 're-education' of the individual patient. The same can be observed in the simple case where no repression is involved. When I have brought my golf swing into consciousness and so discovered what is wrong with it, I have to re-educate my swing, as it were, before I can get rid of my tendency to slice my drive.

There is a point involved in this which is of special interest to philosophy. When the motivation of a neurotic activity is brought to consciousness, it appears as an intention which is not the intention of the agent. He can only

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think of it as an activity of his own by supplying it as it were with an intention which he repudiates. Such an intention is 'unreal'. His real intentions are different and incompatible with it. Now, philosophy is very directly concerned with this distinction between real and unreal, and there is a sense in which all philosophy might be described as an attempt to understand the distinction between the real and the unreal, or, in other words, to understand reality. The attempt to understand the meaning of the distinction proves to be exceedingly difficult, because it seems to be very different in different contexts. Yet unless we know what we mean when we call anything real, we cannot hope to determine what is real and what is not. We shall, in fact, be using the terms 'unconsciously'. The development of psycho-analysis is peculiarly interesting from this point of view, since it draws attention to a use of the distinction which has not been sufficiently considered. There are two familiar uses of the term 'unreal' which have played a great part in philosophical discussion. The unreal has been conceived as the non-existent. It has also been conceived as that which, though in some sense existent, is without causal efficacy; or, to put it in simple language,



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as that which has no practical effect. Now, when we begin to attend to those aspects of human behaviour to which psycho-analysis directs us, we find ourselves dealing with states of mind which we can only describe as unreal. If we find that an otherwise sensible person becomes paralysed with terror if he attempts to cross a bridge over running water, even though he knows perfectly well himself that there is no danger in doing so, we can only describe his terror as unreal. The findings of the psycho-analyst fully justify this use of the term. He discovers that the man is not really afraid of crossing this particular bridge. His fear does not refer to the present situation. He is, to put it paradoxically, afraid of crossing another bridge in his early childhood. In the same way, psycho-analysis may disclose that a patient's persistence in behaving cruelly to some one whom he loves does not *really* express his attitude to the other person, or does not express his *real* attitude, but expresses his attitude to some one else whom he wished to hurt in the forgotten past. There is clearly something in all neurotic behaviour which calls for the use of the term 'unreal' to describe it. But in this case what is described as unreal is neither non-existent nor causally

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ineffective. It exists and it determines behaviour. Indeed, what we have to describe as an unreal emotion is unreal precisely because it is felt and because it does have important practical consequences. This is indeed only a particular case of a general problem, which lies at the roots of philosophy. A belief which is false is an unreal belief. It corresponds to nothing in the real world. It is the product of fantasy. Yet it can only be a false belief if it is actually believed; and when a false belief is actually believed, it can have very important practical consequences. When, therefore, we come to deal with the behaviour of human beings, whether as scientists or as philosophers, we find ourselves facing a kind of unreality which is both existent and causally effective.

We can use this distinction between reality and unreality to describe in common-sense terms the practical problem presented by a case of neurosis. Here is a person, we may say, whose behaviour shows evident signs of unreality. The unreality must arise from the presence of motives determining his behaviour which he cannot control because he cannot bring them to consciousness. He does not know what they are. Perhaps he does not even suspect

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their existence. But he finds himself continually doing things that he can't really account for, and which he doesn't really want to do. He can only be cured if he is enabled to discover them in himself and to overcome the resistances which prevent him from bringing them to consciousness. If he can become conscious of them, he will recognize that they are unreal and will no longer be under the control of their unreality. He will be able once again to behave like a real human being. What is to be noticed in such a statement, whether it is a good description of the situation or not, is that, if we use the terms real and unreal at all, it is the unconscious that has to be connected with the unreal, while the term real is reserved for the conscious intention. The assumption underlying such a description is that the purpose of psycho-therapy is to produce conditions under which a person's real life of conscious intention can control the unreality of the unconscious.

But now we find that this use of terms is reversed when we turn from the practical side of psycho-therapy to the theoretical. The psycho-analyst's ability to do what he sets out to do depends upon a scientific theory of the motivation of human behaviour. This involves

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determining the general components of human motives and the general laws of their combination and functioning. The more scientifically the theory is developed, the more the psycho-analyst must find himself describing the unconscious as the real determinant of human behaviour and attributing unreality, if he uses the term at all, to the conscious. The components of human motive to which he penetrates by analysis are unconscious. The energy by which they express their existence is unconscious. He is driven by his scientific purpose to attempt to describe how this unconscious energy, in accordance with the operation of determinate natural laws, gives rise to consciousness and endows it with its functions and its intentions. In the end this will lead us to look upon the whole of our conscious behaviour as a sort of superstructure built up by the unconscious to achieve unconscious ends. All our conscious intentions will then appear as 'rationalizations' of primary unconscious tendencies. This, however, is another way of saying that all conscious intentions are unreal, since the conscious is actually determined always and everywhere by the unconscious.

It is not the business of the philosopher to pass any judgement upon the scientific validity

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of psycho-analytic theory. But it is his business to draw attention to the dilemma which appears in all the sciences which deal with human behaviour, of which this is a striking example. In psycho-analytic theory the unconscious is the real determinant and the conscious is the unreal, merely apparent determinant of behaviour. The unconscious is here the controlling factor and the conscious the controlled. But in the practice of psycho-analysis this is reversed. From the standpoint of the patient or the psycho-therapist it is the unconscious which is unreal, and the conscious that is real. It is the conscious which is the controlling factor in all normal behaviour, and unreality only appears in human behaviour through the operation of unconscious determinants which are not under conscious control. If, then, we were to take the theory at its face value, we should have to conclude that if the theory were true the practice based upon it would be impossible; and since the practice is not merely possible but practically successful, the theory could not be true. On the other hand, if we took this conclusion to be satisfactory we should be in another difficulty. It is the theory which makes the practice possible. If in practice we dispense with the theory

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because it is incompatible with the practical intention, we should, in fact, have to dispense with the practice also, because it is inseparable from the theory. From a scientific point of view, the success of the practice is a verification of the theory, in spite of the fact that from a logical point of view the success of the practice proves that the theory must be false. It is possible that this antinomy can find its resolution by determining the real nature of scientific verification.

Now, the idea of reality, and therefore the distinction between real and unreal belongs to the level of intentional activity. To speak quite strictly, nothing can be *done* without an intention. Action is inherently intentional. Where there is no intention there are only events which happen. When a person does something unintentionally we call the change that is produced accidental. Accidents happen in the course of our action; they are not integral parts of it. We are not directly responsible for them. If we are held responsible, it is on the ground that if we had been more careful they would not have happened. We have therefore to draw a distinction between actions and events; a distinction which we can mark clearly by saying that events happen, while actions are performed.

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Events are not performed and actions do not happen. Whenever we conceive anything as an event, we imply that it is not an action. In other words, all events are matters of fact; while no actions are matters of fact, but matters of intention. The world of fact consists of things and events. Things are, as it were, the static components of the world of fact, and events the dynamic components. What characterizes this world of fact is the absence of intention and, hence, of action. This distinction between fact and intention is the basis of all practical consciousness. From the point of view of any person who is acting deliberately in the world, his environment is not mere fact. It appears rather as a set of possibilities which limit the intentions which he can realize in action. What, from a purely passive point of view, is mere fact, is, from a practical point of view, something that can be acted upon, and so altered or used in some way or other. The knowledge which is the cognitive element in a practical activity is knowledge of what is possible. It is knowledge of what can be done if one chooses to do it. For the agent, therefore, the real is not mere matter of fact. It is not what exists or what is happening, but the possibility upon which his intention is directed.

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The object of an intention cannot be a fact, since a fact already exists, and an intention is concerned to bring into existence something which does not yet exist. Thus in the world of practical consciousness, which is the primary form of human consciousness, the real is never merely fact, although it is never unrelated to fact. It is always that which is to be achieved or realized by operating upon and modifying fact.

Now, all human action is intentiona<sup>1</sup>, so far as it is characteristically human and, in the strict sense, action. But the intention may itself be theoretical. The primary intentions of human life are necessarily practical; that is to say, they intend changes in the environment. But we can limit our intention, temporarily at least, to theoretical ends. We can form an intention which terminates in the securing of knowledge, and does not look beyond this end to the use of that knowledge in practical activity. When we do this, we turn from action to reflexion, and the essence of this change of attitude is simply that the intention of our activity is limited to changing our ideas. We no longer intend to change the environment but only to change our own consciousness. It is easy to see that when



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we limit our intention in this way to the production of changes in our ideas about the world, the world itself appears at once, relatively to this intention, as mere fact. The world necessarily appears to the reflective consciousness as that which exists and functions in a determinate fashion independently of human intention. It is that which we are seeking to know and to understand, not that which we are seeking to alter or use. At once the distinction between real and unreal alters its colour. Its fundamental significance remains unaltered, but since that refers to the objective of intentional activity the change in the character of the intention involves a different application of the distinction between real and unreal. The real is now the true idea of the world as it is, in distinction from any false idea, which will be the world as it is thought to be, but as a matter of fact is not. In this sense, the world of fact is the general objective of theoretical consciousness. In asserting this, however, we must not forget that the theoretical consciousness is merely the practical consciousness in reflexion, and that the reflexion is brought about by the limitation of intentional activity to the subjective field.

With this understanding of the relation be-

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tween theoretical and practical intentionality, let us consider again the dilemma of psychological science in the form in which it appears in psycho-analysis, as an antinomy between theory and practice. In the practice of psycho-therapy the intention is to produce a change in the environment. There is a difficulty here that is not unconnected with the paradoxes which we have considered. The psycho-therapist is seeking to produce changes in his patient, and the patient is undoubtedly part of the environment from the doctor's point of view. Yet he is primarily concerned to produce changes in the consciousness of his patient, and from the patient's point of view, these changes are not changes in the environment. It is this that makes it illegitimate to talk about 'the environment' in general, because that conception is relative to a particular person and varies from one observer to the next. In particular, each of us is part of the environment for all the others. This, however, is not the point with which we are concerned at the moment, and we can avoid the complications which it introduces by defining the intention of the psycho-analyst as being to produce changes in the behaviour of his patient. He is seeking to eliminate from the

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patient's behaviour elements which are unintentional, and to substitute for them activities which are intentional. In general, therefore, the intention which underlies and directs the practice of psycho-therapy, is the enlargement of the field of conscious intention in the patient's behaviour by eliminating from it the disturbing element of unintentional activity which prevents the patient from achieving his purposes. Since it is intentional activity which is the objective of his practice, it is intentional activity which is real, or, as he will perhaps prefer to say, normal. And it is the activity which is unrelated to the patient's conscious intentions which is unreal or abnormal. On the other hand, when the psycho-therapist turns to the theoretical aspect of his task, his intention becomes a theoretical intention and his patient's behaviour and his patient's consciousness appear as matters of fact or as events which happen in the world which he wishes not to alter but merely to understand and formulate in general scientific terms. The abnormalities of behaviour which are his primary concern are, in fact, things that happen in the course of his patient's behaviour and not things which his patient *does* in the strict sense. It is this that justifies him in

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putting on one side his knowledge that his patient is a conscious human being, in control of and, therefore, responsible for his own behaviour, and in treating him, for the purpose of diagnosis and cure, as a part of the environment to be understood and operated upon. In reflexion, the psycho-therapist asks not 'What is this man doing?' but rather 'What is happening to him?' and 'What is going on in him?' His questions are about events, not about actions; about matters of fact and not about matters of intention. Even if he does ask questions about the patient's intentions, he must treat the intentions as matters of fact, or as events which happen in the patient's mind. This will be true even if he is concerned merely to understand this particular patient and his behaviour. But his scientific intention is more general, and aims at an understanding which shall be applicable to all persons. Thus the particular patient must become merely an instance of a general rule, and the hypothesis which he frames to enable him to understand what is happening in his patient must be in form applicable to all cases of that kind. From this point of view, all human activity appears as matter of fact determined not by intentions but by objective laws

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which are independent of human intentions. Further, what is now real is no longer that in behaviour which realizes an intention, but that in behaviour which is mere matter of fact; and the generality of the formulation demanded extends this to cover the whole field of behaviour, since from the purely theoretical, scientific point of view everything must appear as matter of fact, and not as matter of intention. Thus, from the point of view of psycho-analytic theory it is the unintentional that is real and the intentional that is unreal. All our behaviour appears as matter of fact, and therefore, as determined throughout by unconscious motives. In other words, we never really intend at all. The conclusion is at once logical and absurd. It is logical because the limitation of intention to the production of theory makes the world appear, relative to the intention, as purely matter of fact. Intention has been withdrawn from it. It is absurd, because the theoretical intention is itself relative and limited. If it were absolute, it would involve the denial of all intention whatever, including the theoretical intention itself. If the theoretical intention were valid absolutely and without limits, the intention to understand the world would itself be unreal

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and all knowledge would be impossible.

Thus, in the development of science, we are ultimately brought up against the limits of science. We find that if scientific knowledge is the only possible kind of knowledge which is valid, then all knowledge, including scientific knowledge, must be impossible. This is the full form of the paradox which we have uncovered in different connections in the earlier parts of our investigation. Its palpable absurdity can only mean that scientific knowledge itself is relative to a kind of knowledge which is not scientific; which is indeed, as we can now see, a knowledge, not of fact, but of intention. Since action is intentional, this will mean a type of knowledge which is not limited by the withdrawal of intention from the production of a change in the external world; a knowledge which is not purely theoretical, but is integrated with practice and so belongs to the practical consciousness. In such knowledge, theory will itself necessarily be contained in practice and not separated from it and contrasted with it.

We shall consider certain aspects of this conclusion in our final chapter. For the moment we need only notice that it fits in with the view which was suggested by our consideration of the

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sociology of science, that the limits of science are the limits of instrumental knowledge. We have now seen that the unconscious elements in human action are, in principle, those elements which are instrumental to the achievement of a conscious intention. In so far as we must use the structure of our own selves, including that structure of habit which is the deposit of our earlier activity, for the achievement of our intentions, we require a knowledge of how it can be used. There is, therefore, a personal and a social need for a scientific psychology, which will deal with human behaviour, whether in ourselves or in others, in so far as it is matter of fact. But just as any science presupposes conscious intentions by the mere fact that it sets out to provide the knowledge through which alone they can be realized, so a scientific psychology presupposes human intentions which require for their realization the use of the established habits and other automatisms of human nature. Any intentional activity moves from the present towards the future. All that exists in us as a result of the past, whether that past was a product of intentional activity or not, is now mere matter of fact which we must accept, and through which alone we can

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determine the future. Since it is matter of fact, it can be known, and properly known, as matter of fact. Since it is dynamic, it expresses itself in tendencies to continue into the future the forms of activity which have been established in the past. From this point of view it constitutes an organization of motives which are at once relatively determinate and automatic. Intention, however, is concerned with the future, and from the point of view of intentional consciousness all this is instrumental. It is our existing means of determining, not what is, but what shall be. The antinomy between the pure theoretical consciousness and the practical consciousness is resolved for our understanding when we remember this, although it is not abolished. The paradox itself is seen to be natural and necessary.

There is a sense in which psychology teaches us how to behave, though it is a limited sense—limited by the instrumental character of scientific knowledge. The purpose for which we behave, since it is a presupposition of science, since it refers to the future and is not matter of fact, cannot be part of the subject-matter of psychology. If I ask, 'How shall I behave?' I may mean either of two quite different things. I may mean, 'What intentions shall I form and seek to realize



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in action?' or I may mean, 'Seeing that I have such and such an intention, what shall I do in order to realize it?' I may mean either, 'What shall I intend?' or 'What means shall I take to realize my intention?' The first question considers my behaviour as consisting in the formation of intentions; the second as consisting in realizing intentions which are presupposed. It is only this second question with which a scientific psychology is concerned. The answer to the first question lies beyond the limits of scientific knowledge. This resolves the dilemma of science. When we realize that the limits of science are the limits of instrumental knowledge, we see that scientific psychology is knowledge of human behaviour in so far as it is matter of fact, in so far as it can be the object of pure theory, in so far, that is to say, as it is 'unconscious'. Since there is no possible human activity which has not an unconscious aspect, there is no human behaviour which cannot form part of the subject matter of scientific investigation. Yet the conscious, intentional aspect of human behaviour can never be the object of scientific investigation. It is at once true that all human behaviour can be investigated scientifically, that there is no human activity which is ex-

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cluded from the field of scientific psychology, and also that there is an aspect of human behaviour which must necessarily escape completely from the account that science gives. And it is indeed precisely this aspect which makes human behaviour specifically human.

## Chapter 6

### FACT, MOTIVE AND INTENTION

In concluding this examination of the psychological sciences we are concerned with the philosophical conclusions which are implied by the antinomies which it has brought to light. Some of these have been anticipated more or less fully in the course of the discussion. Others have been merely suggested or implied. We must now attempt to decide what is the central conclusion to which the whole argument leads.

The difficulty in doing this is considerable. It is easy to draw conclusions which seem on the surface to be warranted and yet which are neither necessary nor justifiable. It would be easy, for example, to represent the argument as an attack upon the validity of psychology or indeed of science as a whole. Surely, one might think, if it can be shown that a science leads in-

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evitably to conclusions which are so paradoxical that if they are true they cannot be true, it must follow that such a science is self-condemned by its own inherent contradictions. Should we not at least draw the conclusion that a scientific psychology is impossible? Alternatively, we might adopt the role of partisans of science and conclude that if philosophy leads to such fantastic conclusions about science, it is philosophy that has condemned itself and revealed that its methods lead to conclusions which are certainly untrue. Neither of these conclusions, however, is warranted by the evidence. They rest upon concealed assumptions about the nature of knowledge and the nature of truth. The validity of science, it seems to me, is not in question, even in the field of psychology. The validity of particular scientific theories is, of course, always in question, but the decision in that case is one which only science itself can decide. The testing of the validity of any scientific hypothesis is itself a scientific activity. It seems to me as absurd as it can seem to any scientist for a philosopher to assert that science is not valid knowledge. On the other hand, I can see no reason for conceding to science the right to pass judgement on philosophical questions. The ten-

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dency of philosophers and scientists to fall into futile discussions of this kind, which are only too apt to degenerate into slanging-matches, seems to arise from an assumption that philosophy and science are both investigating the same kind of question and that the kind of validity that is aimed at in both cases is the same. The assumption that there is only one kind of knowledge of the world is, after all, an assumption. We assume too readily that in all cases we know what we mean when we assert that a statement is true, and that we always mean the same thing. Yet, the problem of the meaning of truth is one of the perennial and unsolved problems of philosophy. It is at least possible that the solution of our problem lies in the fact that this is not the case, and that where a scientific statement and a philosophical statement contradict one another, it is because they are answers to quite different types of question.

There is one kind of contradiction which illustrates this possibility very clearly. A statement may be true as a matter of fact and yet false as a definition. The statement, for instance, that 'a man is a material object' is true as a matter of fact. But if it is taken as a definition it is false. Where the purpose of the statement is to define;

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that is to say, where the statement is an answer to the question 'What is a man?' the answer 'a man is *not* a material object' would be valid. This is not without its bearing upon the issue between science and philosophy. It is quite possible for a biologist and a philosopher to fall into a heated argument over the question, 'Is man an organism?' and the dispute can only be brought to an issue by recognizing that the biologist is making the statement that man is an organism as a statement of fact, while the philosopher is considering it as a definition. In other words, each of them is taking the assertion as an answer to a different question. At bottom, the dispute arises from a difference in intention. The intention of the scientist in making the assertion differs from the intention of the philosopher. The validity of the statement is relative to the intention which gives rise to it.

Consider another type of case. There is a discrepancy between a scientist's description of any material object and any ordinary description of it. The common-sense description of a table represents it as a solid, static object. The scientific description represents it as consisting of minute particles at relatively great distances from one another, and all in perpetual and

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rapid movement. These two descriptions are clearly contradictory; so much so that one finds scientists talking as if there were two tables, the scientific table and the perceived table, and one finds philosophers arguing the question which of these two tables is the real table. Yet the pre-supposition of all this is that both are descriptions of one and the same table and that there are not two tables but one. If, for example, we adopted the conclusion that the scientist's table was the real one, we could not also maintain that the scientist's description was the true description of the table we see. Yet, the scientist's description of the table claims to be valid because it is based upon observed fact, and in this case the observed fact is what is expressed in the common-sense description. If the scientist's description is to be substituted for the immediate deliverance of normal sense-perception, it is difficult to see how it can claim to be based upon observation. It is the scientific description that is the derivative one. If the validity of the direct description of what is perceived is denied, the validity of the derivative description must be denied also. Either both descriptions are valid or neither is valid, since they depend upon one another. On the other hand, they are contra-

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dictory, and if both are to be valid, they must be valid in different senses. Both must be proper answers to different questions.

This particular example illustrates another issue which is directly pertinent to our main question. A table is an artificial object which is the product of purposive human activity. For this reason, it cannot be properly defined without reference to the intention which gave rise to its construction. But a scientific description of the table must not contain such a reference. The scientist will describe a table in a way that would be equally applicable to a natural object. His description would apply equally to any natural object of the same material. Consequently the scientific description is not strictly a description of a table at all, since what makes a table a table is that it is constructed and used for certain purposes. The scientist's description is, however, perfectly valid, even though it cannot be adequate as a definition. Here again we notice that an aspect of the object is omitted from the scientist's description, and that it is an aspect which is bound up with the intentionality of human behaviour.

Such cases make it clear that the purpose of scientific investigation already limits it to a cer-



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tain aspect of the world which it studies. This can be seen from another point of view if we notice that science is peculiarly concerned with events rather than with things. No doubt science has to deal with things, and up to a point with their constitution or nature. But this is not its real interest nor its goal. In general, it is not concerned to discover what things are, but rather to discover how things behave. This is the significance of the notion that what interests science is the question of causation. The laws which it seeks to formulate are explanations, or if that word smacks too much of intention and purpose, they are at least descriptions of how things happen. If we are to have any satisfactory knowledge of the laws which describe the behaviour of things, we must, no doubt, have some knowledge of what they are, at least sufficient to identify them and distinguish them. But we can understand how things behave—especially if we are prepared to abstain from asking why they behave as they do—without any very deep or adequate knowledge of what they are in themselves. A working hypothesis will suffice, and we need not concern ourselves too closely with its accuracy so long as it serves our purposes. A good example of this may

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be found in the recent transition in physics to Schroedinger's wave-mechanics in atomic theory. As descriptions of the structure of the atom, the wave theory and the corpuscular theory are totally incompatible. They are both, as it were, pictures of what an atom would look like if it could be sufficiently magnified for its inner structure to become visible to the human eye. No one imagines that if this were possible an atom could appear both as a system of definite particles revolving in orbits round a central nucleus and also as a complex of ripples in a fluid. Either might be possible, but not both. If the question which is the true picture is raised, the scientific answer is simply that this question needn't be answered. It certainly does not trouble the scientist at all. For that reason he is likely to suggest that the structure of the atom can be properly described only in mathematical formulae, and that all attempts to construct visual models are misleading, even if they may sometimes be helpful. A mathematical formula, however, is unfortunately not a description at all, and the readiness with which the scientist will substitute one picture for another merely indicates that he is not really concerned with the validity of a description of the constitu-

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tion of matter, and is ready to adopt any description which proves adequate to another purpose—the formulation of laws which describe, not the constitution, but the behaviour of things. For himself the validity of any description of the inner constitution of the atom is purely pragmatic. To put it crudely, it does not matter to science whether there really are electrons or not, provided that things behave as if there were. In the case of psychology this would mean that it does not really matter whether the psychologist's description of the human unconscious and its constitution is accurate or adequate, provided human beings behave as if it were. It may be adequate for the scientific purpose of determining the laws of observable human behaviour. Nor does this mean that a scientific hypothesis of the constitution of the atom or of the human unconscious is not valid. It means that its validity is relative to a purpose, and that purpose may be purely theoretical, in terms of truth. If it were not valid, then one working hypothesis would be as good as another, which is certainly not the case.

It is clear, then, that the interest of science in the world it studies is limited. There are all sorts of questions which it does not ask and is

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not interested in answering. There are also considerable sections of scientific theory which are treated rather light-heartedly by the scientist himself, as though the question of their truth were of no essential importance to his purpose. In general, it would seem that the focus of scientific interest is the behaviour of things, and its essential aim seems to be to determine the general principles of their behaviour, or what one might call the fixed habits of their natural behaviour. This attitude of mind is easy to account for if we turn to the feature of practical experience which provides a motive for the co-ordinated effort to produce science. In our ordinary experience of life, so far as it is necessitated by the natural conditions of life, the knowledge which we need to live by involves an element of foresight. The achievement of our purposes depends upon our ability to foresee the future. This element of foresight is in principle contained in all cognition, and there is no reason to doubt that we share it with the higher animals at least. Intelligence, which is primarily displayed in behaviour, always has a reference to the future, and it contains an element of anticipation. The adaptation to the environment is always an adaptation to the environ-

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ment as it will be, even if the span of the anticipation is extremely small. When a cat pounces on a mouse it has to spring at the spot where the mouse will be when it lands. What is characteristic of human life is the necessity for a much longer span of anticipation than is involved in the behaviour of animals.

Up to a point, this capacity for anticipation develops naturally without a deliberate effort of reflective thought. But there is a point beyond which any further development of our power to anticipate the future will not develop naturally. Beyond that point any further development must be the result of a conscious intention to increase its range, and the discovery by reflexion of methods for doing so. Thus, a purpose which can only be realized through a set of activities which last over a considerable period of time can only be achieved, or indeed adopted, if there is a corresponding power of anticipating the future. If I wish to cross a road through a stream of traffic, I do so by anticipating the position of moving vehicles at the moment when I shall reach the part of the road along which they are moving. I can do this without hesitation or any conscious effort of reflexion provided the situation is one with which my experience

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has made me familiar. If I am a peasant on my first visit to a busy town, I may find it very difficult or even impossible. But suppose that I wish to fire a machine-gun through the blades of the propeller of an aeroplane when it is flying. The problem is precisely the same in type. It is a question of anticipating the position of the moving blades at the time at which the bullet from the machine-gun will pass through the plane in which they revolve. But the high speeds with which we are now dealing require an anticipation which must be exact to very small fractions of a second and to very small fractions of an inch. Such exactness of anticipation cannot be reached by the development of the kind of anticipation which is involved in sense-perception. In fact, the human eye is quite unable even to distinguish the blades of the propeller as separate entities when they are revolving at high speed. The anticipation is only possible through the development of mathematical calculation; and to give effect to the intention involves also a knowledge of the material world of a kind which will enable us to construct a physical device which will operate with the exactness required. This is the first point we must notice. Scientific knowledge is essentially

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a development of our power of anticipation through the development of reflective methods of calculation.

The second point concerns the methods themselves. Pure mathematics is itself the development of our natural ability to count. But that natural ability is limited in ordinary experience to the enumeration of objects which are distinguishable as separate things by the senses. Mathematics is, again, the construction of devices for carrying this capacity beyond the limits of sensory experience. It involves the elaboration of techniques for counting things which are not distinguishable by the senses. In some cases what is so counted is known to consist of distinguishable elements, although our senses unaided cannot distinguish them; as, for example, the spokes of a wheel in rapid motion. In other cases what is counted may or may not be made up of separate elements, and the assumption that it is is a hypothesis of greater or less probability. The assumption of the atomic structure of matter is a case of this kind. But there are other cases in which what is counted is known not to consist of distinct elements. This is the case in pure mathematics where space and time are counted and measured. We know that space and time are continuous, and

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that to treat them as if they were made up of distinguishable parts leads to contradictions in thought which are unavoidable. In counting space and time we know that we are not describing it as it really is, and because we know this we can make allowances which cancel out the effects of our deliberate error. We can measure the circumference of a circle by thinking of it as if it consisted of a large number of straight lines. By the device of saying that there are an infinite number of these straight lines in the circumference, we cancel out the error by another error. If there were an infinite number of straight lines in the circumference of a circle each of them would have precisely no length, and hence would not be a line at all. It is clear that the mathematical analysis of space and time is not a true description of space and time as we know them. It is, instead, a reflective device of great ingenuity which achieves its purpose with absolute exactness. The conclusions which we are enabled to reach by using it are true. But if we were to conclude that the mathematical analysis was itself a true description of the nature of space and time, we should be manifestly wrong. We know that space and time do not consist of small particles which can be added



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together to make up the whole of space and time. But we know also that we can treat them as if they were, and that if we do, we can achieve knowledge which enables us to anticipate events which will occur in space and time with extreme accuracy. In other words, the reflective device of mathematical calculation enables us to answer correctly questions about things which really are countable, and its validity consists in the fact that it does this, not in being a true description of anything at all.

We may generalize this by saying that in all scientific knowledge we must distinguish two elements, one of which is knowledge proper, and the other of which is an element of construction, a device which enables us to reach the conclusions which are the elements in our scientific knowledge of the world, but which in themselves may or may not correspond to the real nature of things. Their truth or falsity, if they are taken as assertions about things, is in fact irrelevant. They are simply devices for carrying the processes by which we achieve our anticipations of the future in ordinary perceptual experience beyond the limits of possible perception. A good example of the element of construction in scientific theory is the theory of the grouping of the atoms in the

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complex molecules of organic compounds. These elaborate pictures of the patterns of arrangement of atoms are essentially devices which enable the organic chemist to anticipate or predict the observable behaviour of the substances with which he deals. Whether they are true descriptions of what we would observe if we could observe the structure of these molecules, we do not know, and no experiments that we can make can tell us. But from the scientific point of view the question whether they are true descriptions or not is irrelevant. Their validity consists in the fact that they are satisfactory devices for anticipating what can be observed.

There are two essential points to be noted here. The first is that experimental verification does provide us with good reasons for believing in the truth of the conclusions which are reached through these devices of the constructive imagination, and at the same time provides us with evidence of the validity of the constructions as devices. It does not, however, in the least warrant a belief that the constructions themselves are true descriptions of the unobservable structure of the things which they represent. The question whether organic substances in fact are composed of molecules and atoms, as well as

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the question whether these hypothetical atoms are arranged in any particular way in the hypothetical molecules, is strictly irrelevant to scientific knowledge. If the scientific picture of the world which is provided in these constructions is accepted as a true description of the actual structure of things, we turn it into a mythology. The second point is equally important. The statement that the truth or falsehood of these scientific constructions is irrelevant to scientific knowledge does not mean that the question of their truth is meaningless. Whether we can answer it or not, it is a perfectly valid question. And it does not follow that because it is irrelevant to science, these scientific descriptions, or some of them, may not be true. The probability is that some of them are true as descriptions and that some of them are false. Indeed, in some cases we know that they are true. In others we know they are not. But for science it does not matter. Their scientific validity consists not in their truth as descriptions, but in their satisfactoriness as devices for reaching conclusions which can be verified by observation; that is to say, by successful anticipation of observable behaviour.

Now let us apply this general conclusion

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about science to the special field of psychology. In psychology, since it is a part of science, the same distinction between the element of verifiable conclusion and of imaginative construction must be made. The psychologist is interested in human behaviour. It is the overt, observable behaviour of human beings about which he seeks knowledge and concerning which his conclusions are verifiable beliefs. The verification consists, as it must, in the capacity to carry the anticipation (and so, ultimately, the control) of human behaviour beyond the limits which our unreflective experience can reach. In order to do this, he must have recourse to the construction of devices for reaching conclusions similar to those we have noticed in their simplest form in the construction of pure mathematics. He constructs for this purpose a picture of the unobservable structure of the human mind. He divides it into the conscious and the unconscious mind and he constructs a detailed picture of elements of which each is composed, and of a dynamic relation between the different elements which he distinguishes. All this is imaginative construction, comparable to the organic chemist's imaginative picture of the internal structure of organic molecules. The

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validity of his picture lies purely in its satisfactoriness as a device for anticipating observable human behaviour. It does not lie in its truth. The question whether his construction is a true description of the unobservable structure of the human mind is irrelevant from any scientific point of view. So long as it works satisfactorily as a device for reflective calculation, that is all that is required of it. If the psychologist, or any one else, accepts it as a true description of the human mind as it actually exists and functions, he is turning it into a mythology. On the other hand, this does not mean that some parts at least of the construction may not be true descriptions, but only that for purposes of scientific knowledge they are equally valid whether they are or not, provided that they fulfil the function for which they are constructed. Scientific psychology is not in a position to answer the question about the real inner structure of the human mind, neither does it need to. The notion of the unconscious may be as inherently impossible as the notion of an infinite number of straight lines in a finite length. If it is, that will make no difference to its validity as an element of necessary construction in the psychologist's work. If, therefore, we should find reason to conclude

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that the psychological analysis of the human mind could not possibly be a true description, we should not have disposed of scientific psychology. It would still remain totally unaffected, since the question of the truth of this construction is irrelevant. And the conclusions of psychology would still be valid knowledge and guaranteed by the increase in the scope and the exactness of the anticipation of human behaviour which psychology makes possible.

This recognition of the part played by the element of construction in science would enable us to understand the dilemma which arises in psychology, were it not for the fact that we know that the psychologist's account of behaviour itself is inadequate. Even if we make the fullest possible allowance for the irrelevance of the question whether the descriptive devices used in psychological theory are true or false, we still are faced with the peculiar difficulty which arises from the inability of our scientific theory to distinguish between matter of fact and matter of intention. We are directly conscious that our behaviour is bound up with our effort to realize our intentions and determined by it. But scientific psychology must exclude this element of intention from consideration and des-

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cribe our behaviour itself as matter of fact, the origin of which must be looked for not in our intentions but in the world of fact. We have already seen the reason for this. The object of scientific investigation must be looked upon as part of the 'external' world; that is to say, as independent of the conscious intention of the agent. When I observe the behaviour of other persons, this is how it appears to me. The element of intention is not observable. But at the same time I have a direct knowledge of my own behaviour which is of a different order, in which it appears as the realization of my purposes. This distinction is marked in our natural forms of speech by the distinction between actions and events. An event is matter of fact. It happens in the world. If, therefore, I wish to understand its happening, I ask for the cause of it; and I must seek for the cause in other events with which it is connected in some necessary fashion. An action, however, is not something that happens, but something that is done. If I wish to understand it I ask why it was done, and the answer to this question is a statement of the intention of the agent. It is in this sense that an action is matter of intention and not matter of fact. When psychology adopts the scientific attitude

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in its inquiry into human behaviour, it treats human behaviour as matter of fact, not matter of intention. In other words, it does not strictly consider human action at all. It looks upon any instance of human behaviour as an event which can be observed to happen, not as an action which is done, and consequently it must proceed, in its attempt to understand human behaviour by asking what is the cause of it. The answer must be in terms of matter of fact, that is to say, of other events which condition and account for its happening. Consequently, in psychological terminology what we know directly as action becomes behaviour, and what we know directly as the intention of our act appears as the motive of our behaviour. This introduces a possibility of continuous ambiguity. As we have seen, conscious activity has an intention while unconscious activity has no intention, though it has a motive. The term behaviour covers both cases, and the term motive, in turn, can be applied to both cases, though when it is applied to intentional behaviour there is considerable difficulty in distinguishing between the motive and the intention of the action. We find ourselves constantly using the terms 'motive' and 'intention' as if they were synonymous.



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It would seem, therefore, that the question whether, and within what limits, scientific psychology can provide us with a true understanding of human behaviour, is bound up with the question of the relation between motive and intention, and so with the relation between what is matter of intention and what is matter of fact.

A motive, in the most general sense, is simply that which moves anything. We are familiar with this sense of the term in reference to natural forces. The wind is the motive force which drives a sailing boat. Electricity is the motive force which drives an electric motor. If I consider the case of a man driving a motor-car I find that I can use my terms ambiguously. I can say that the man at the wheel drives the car, or that the engine drives the car. Unless the engine is in working order, and supplied with petrol, the car cannot be driven. There is no motive force to make it go. On the other hand, the car cannot be driven without a driver. It could no doubt be set going and move without a driver but in that case it would cease very quickly to be a motor-car. There are two distinguishable elements in the driving of a car, the motive force which propels it and the ac-

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tivity of the person who directs it. Now, in any human action the same two elements can be distinguished. Unless there is a sufficient store of physical energy in the human body no action is possible. On the other hand, unless there is an intention which directs the expenditure of energy, there can be no action either, but at the most only spasmodic or automatic movements. The motive of an action is, at least in part, the physical energy which is available, and without which bodily movement is impossible. There are indeed states of mind in which a violent effort of will is incapable of determining action, as when some one is literally paralysed by fear. This element in the motivation of human behaviour is necessarily 'unconscious', and its relation to the action which it makes possible is quite strictly a causal relation. There is clearly a sense in which it would be possible to account for human behaviour by reference to the transformation of energy involved in the movements of the human body. Such an account would be precisely similar to the account one could give of the movement of a motor-car by reference to the transformation of energy in the engine and its transmission to the wheels. It would be, in principle, a valid account; but no

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one would contend that it was an adequate account.

There is another element in the motivation of human behaviour which distinguishes it from a merely physical motive force, and which involves the presence of consciousness, though not necessarily of intention. I refer to the sense in which fear of anger or jealousy may be the motives of human behaviour. A man may be so angry that he is unable to control himself. He may find that against his conscious intention his anger determines his behaviour. In such cases the action has a motive in consciousness and yet is not the realization of an intention of the agent. The distinction between motive and intention is clear in such an example. It is complicated, however, by the fact, that a person may be mistaken about his motives in this sense of the term. He may be under the influence of anger or jealousy without knowing it. Here we come upon one of the most distressing ambiguities in the meaning of the term 'conscious'. We are apt to say that such a person is jealous without being conscious of it. This does not mean that he does not feel the emotion that we call jealousy. If we include feeling in the meaning of the term 'consciousness', then his

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jealousy is in consciousness. He certainly feels it. On the other hand, he does not recognize it as jealousy, and may be quite sure that what he is feeling is righteous indignation. There is a sense in which our emotional consciousness is fundamentally a motive consciousness, as our intellectual consciousness is not. The recognition of the emotion that we feel and which provides the motive for our action is more than the feeling of the emotion. It involves a reflective, cognitive act of the mind of which the emotion felt is the object. It involves the expression of the emotion in a word or some other symbol—the production of an idea which refers to and is distinguished from the emotion itself. Such a reflective expression of what I feel is liable to error. In that case I am feeling an emotion but I do not know what it is. I may even be convinced that it is a different emotion. The emotion itself, however, acting as a motive, will express itself in my behaviour, and I may recognize my mistake by seeing the objective results of what I have done and recognizing them as the expression of jealousy. Again, I may not; yet other people observing my behaviour may have no difficulty in recognizing it as the expression of jealousy, even when I would indig-

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nantly repudiate<sup>1</sup> the suggestion that I was jealous. In one sense, it would be proper to say that in such a case my jealousy was unconscious, though in another sense it would be proper to say that it was conscious. I mean by conscious in the first case the recognition of my emotion for what it is. In the second case the fact that I feel it. The psychological distinction between the conscious and the unconscious often seems in this way to be a distinction between the motive or emotional consciousness and the reflective, cognitive consciousness; so that when a motive is said to be unconscious this does not mean that I do not feel it, but simply that I do not recognize it; or, at other times, that I cognize it incorrectly. In that case, there is a symbol in my reflective consciousness which does represent the motive which I feel but which represents it incorrectly. I do not understand my motive, I misunderstand it. I think it is what it is not.

When motives, in this sense of the term, are the causes of human action, they are not motive forces. They presuppose the existence of a store of physical energy which is available for bodily movement. But they determine the direction in which this energy shall be expended, in

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response to stimuli from the environment. It is possible, and within its limits legitimate, to account for the behaviour of human beings by determining the motives of which it is the expression; and since these motives do not imply intention, such an account of human behaviour will not involve any reference to human will, and can, therefore, be strictly scientific in its attitude. This determination of the motivation of human behaviour seems to me to be the proper field of scientific psychology, since the question which it seeks to answer is a legitimate question and one to which the scientific method of observation, hypothesis, and verification can, in principle, be applied. Further, it seems in principle to be applicable to all human behaviour without exception. For an intentional act depends for its possibility upon the presence of adequate motive in this sense of the term. It will not, however, be a complete or adequate account of intentional behaviour, since it abstracts from its intentionality and confines itself to the consideration of motives, which are matters of fact. It is only if such an inquiry forgets the limitation of its investigation to matter of fact, and proceeds illegitimately to deny intentionality, or to reduce it to matter of fact by

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confusing intention with motive, that it falls into error.

The motives which make possible the performance of intentional acts are, as we have seen, the result of the formation of habits in the earlier history of the agent. Habits are, in fact, those tendencies to respond to particular types of stimulation in determinate ways which have been established in the agent in the process of his practical experience. They provide a pattern of behaviour in the individual which expresses itself in the recurrence of similar acts in similar situations. Since they are adaptations to the environment, and since the environment is to a great extent the same for all human beings, there are naturally habits which are more or less general for all human beings. Where, therefore, there is a habit of behaviour it is in principle possible to determine its pattern and express its recurrence as a natural law of the scientific type. Whatever in such habits is necessitated by the universal conditions of all human action, can, therefore, be properly expressed as a universal law of human behaviour. Thus, there is no reason in principle to deny the possibility of discovering universal laws of human behaviour of the same type as are established

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by other sciences, however 'great the practical difficulties in the way of their discovery may be. Their discovery and formulation has no bearing upon questions of will and intention, since the inquiry abstracts from this aspect of human behaviour from the start. The contradictions which may arise between the statement of these laws and the results of an inquiry into the intentionality of action are, in the last analysis, merely verbal. They arise from the limitation of the point of view which restricts itself to the investigation of the *de facto* motivation of behaviour.

Intentional action presupposes habit and, so, the existence of adequate motive for the realization of an intention which the agent has formed. For this reason, the systems of motivation which the psychologist seeks to understand, are a necessary means to the performance of conscious action. Scientific psychology is, therefore, an investigation of the means of intentional actions. This is precisely the sense in which we define scientific knowledge as instrumental knowledge, and we can, therefore, define scientific psychology as the effort to achieve an instrumental knowledge of human behaviour. When we recognize that our ability to realize our in-



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tentions in action depends ultimately upon our ability to use a *de facto* motivation of behaviour which is available through the habits of reaction to the environment which experience and training has established in us, the immense importance of a scientific psychology must become evident. The understanding of the motivation of our behaviour must be the basis for any extended control of it in the interest of rational activity. At the same time our capacity to use this knowledge as it is gradually achieved, must depend upon our recognition of its instrumental character, and so of its limitations as an account of the nature of human action. The paradoxes which we have discovered in our investigation remind us forcibly both of the necessity of this and of the ease with which we fall into the error of confusing an account of the motivation of behaviour with an account of the real nature of human action. It will be well, therefore, to conclude by drawing attention to the nature of the distinction between fact and intention.

Any satisfactory account of human behaviour must be, in some sense, teleological. This is true, indeed, of the behaviour of all living creatures. What is often overlooked is that the idea of

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teleology, in its modern use, is ambiguous, and conceals a distinction in kind between intentional action and organic behaviour. To say that organic behaviour is teleological does not imply the presence of purpose as a determinant of the behaviour. It implies merely that the behaviour is in the strict sense a development, and not a mere series of changes; that it is directed activity which can only be understood by reference to some end which it actually achieves. In this sense, the growth of an acorn into an oak tree is a teleological activity. It can only be described in teleological terms. The teleological description, which makes the tree the 'end' of a process of development which begins with the acorn, does not attempt to answer the question why the acorn grows but merely to express in adequate terms *that* it does and *how* it does. It does not imply that the acorn—or that anything else for that matter—has a purpose which is expressed in its behaviour or even a motive in the psychological sense of the term. To imply this is to confuse teleological with intentional activity. The difference may be expressed by saying that the teleological character of organic behaviour is pure matter of fact, and not in any sense matter of intention.

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The distinction may be brought out in another way. In teleological activity which is non-intentional it is in principle possible to determine the specific character of the activity by reference to an end which is actually achieved by it. The instinctive behaviour of animals does, in fact, achieve organic ends, and can only be understood in terms of the end which it achieves. The building of nests by birds or the construction of the honeycomb by bees do achieve ends and can only be accounted for in terms of these ends, although there is no reason to suppose that the bees or the birds know what they are doing or why they are doing it. There are motives which produce these activities, and in some sense we must say that the end is implicit in the motive. The same is true of all the automatisms in human behaviour. We are continually performing actions which do in fact achieve certain ends without being conscious of these ends. Every step that we take involves an unconscious adaptation of our movements to the necessity of maintaining our balance; but we are normally quite unconscious of this. In intentional activity, on the other hand, the end in terms of which the activity must be understood, is not an end actually achieved. It is not

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matter of fact. It does not develop from the earlier stages of the process and appear as the result. The intention comes at the beginning of the activity and guides it. In this case the 'end' is the starting point of the activity and it accompanies the activity throughout. If it disappeared the intentional activity would cease. Moreover, the intention is not necessarily achieved at all. The action taken may be misguided, and what is the *de facto* end of the action is in that case quite different from the end which was intended. Indeed, reflexion may often reveal that the action could not have achieved the end for which it was undertaken.

The consequence of this is that an intentional action is capable of being described as right or wrong; while in the absence of intention such a description is essentially meaningless. If I have acted in a certain way in order to realize a certain intention, and have instead produced a change which does not achieve my intention, then I have done the wrong thing. For this reason a description of action which limits itself to the determination of the motives of my behaviour describes my behaviour in a fashion which makes it meaningless to call it either right or wrong; and, as we saw earlier, if the

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intention is to secure knowledge, and the activity set going is then described as matter of fact, it will be meaningless to say that the conclusions I have reached are either true or false. Such a distinction implies that my activity in reaching my conclusions has been rightly or wrongly carried on. Apart altogether from any empirical consideration of psychological theories of behaviour, this recognition of the difference between the motive and the intention of an action would enable us to conclude *a priori* that a strictly scientific theory of human behaviour would be such that if it were true it could not be true; since the account which it gives is a purely *de facto* account which abstracts from intentionality. Thus, an intrinsic knowledge of action cannot be scientific, since it cannot abstract from the intentionality which is the distinctive feature of human action. In addition, it must be an account which is concerned to define actions with reference to their rightness or wrongness.

We come back finally to the necessity of a knowledge of action which is not and cannot be scientific, because it must be intrinsic and not instrumental. We need a knowledge which will enable us to act rightly. On such a question,

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scientific psychology can have nothing to say, since the question is no longer a question of fact but a question of intention. It will not be denied, I think, that the knowledge of how we can control our behaviour for the achievement of intentions presupposes the existence of intentions which we may attempt to achieve. What may be denied is that there can be any knowledge in this field. We may represent our intentions as being fixed and unalterable, and all questions as to which intentions are right and which are wrong as inherently meaningless. This is the view which is ordinarily expressed—and by no means well-expressed—when people say that values are subjective, or that they are matters of taste. If this were so, there could be no knowledge of value. The only knowledge possible would be scientific in type, confined to matters of fact. This knowledge would still be instrumental, since it would enable us to achieve our intentions; and the existence of the intentions would make it possible to describe the actions undertaken to realize them as right or wrong. In that case the rightness of the act would lie in the fact that it did achieve its intention.

This view seems to me still to involve a confusion between a motive and an intention, be-

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tween matter of intention and matter of fact. It implies that an intention is a subjective fact. If it were, an intention would not be an intention but a motive, and the distinction between right and wrong, even in the limited sense, would be impossible. It is not difficult to see that an intention is not a subjective fact. It is true that when I intend something my intending it is a subjective fact; but *what* I intend is a change in the objective world, and it is this future change in the objective field that is the intention of my action. It is obviously wrong to describe a future change in the objective world as a subjective fact. But it is also wrong to describe it as a fact at all. This can be seen most easily in the case where an action fails to realize its intention. In that case the future change in the objective world, which was the intention of the act, is not realized and never becomes fact at all; yet this does not imply that the action had no intention. The intention of an action is not then matter of fact. It is not an event in the mind which can function as the cause or motive of a movement of the body which in turn can cause other changes in the objective field.

This, however, though it may uncover the

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source of confusion in our ordinary discussions of the possibility of a knowledge of value, does not answer the question whether a knowledge of value is possible. Indeed, if we think of matter of fact and matter of intention as two different fields of investigation it is difficult to see how there could be a knowledge of value at all. But the reason for this is that we are then unconsciously treating matter of intention as if it were another kind of matter of fact. The relation is different. There could be, indeed there certainly is, behaviour which is unintentional, and which, therefore, is simply matter of fact. But there is no intentional behaviour which does not involve and include behaviour which is matter of fact as an aspect of it. Knowledge which included a knowledge of intentionality and, therefore, of value, would have as its subject-matter not something different from matter of fact, but something more than matter of fact and inclusive of it. It would be an intrinsic knowledge and not a merely instrumental knowledge. In other words, an intrinsic knowledge of the world, and an instrumental knowledge of the world are two kinds of knowledge of the same world, not knowledge of two different worlds. They are, as we saw earlier, distinguishable



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only in reference to the full practical experience of agency, as knowledge of the World-as-Means and knowledge of the World-as-End.

The condition of a knowledge of the World-as-End would seem to be that it should be possible to know the world as itself the embodiment and expression of intentionality. This is the view of the world which is the basis of all theism, though whether it necessitates a belief in God, or in what sense, we need not discuss in this connection. All we need note is that the hypothesis of an intentionality in the world makes it at once possible to discuss the rightness or wrongness of human intention, and to relate this to our knowledge of the world as matter of fact. For it gives us a ground for relating the particular intentions which human beings seek to realize both to one another and to the common world in which the effort to realize these intentions must be carried on. It provides us, likewise, with a fundamental principle of judgement, because finite intentions which run counter to the intentionality of the world in which they must be realized are necessarily self-negating.

Apart from this fundamental postulate, which is at least as well grounded as any of the postu-

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lates upon which our scientific knowledge ultimately rests, we are faced with the social problem of the diversity of human intentions and the necessity of finding a resolution of the practical contradictions which they often involve. The reconciliation of intentions, the harmonization of wills, and not merely of motives, is the basic condition of any satisfactory social order. Its possibility within limits is demonstrated by the fact of human community and by the peculiar skill which certain people show in mediating between persons whose wills are at enmity and bringing about a reconciliation. There is surely involved in this a kind of knowledge of the intentionality of human action which provides a basis for a form of systematic reflection which could extend and deepen the possibility which it reveals. At least we may reach one negative conclusion which seems to be indubitable, and which events in our own generation are forcing increasingly upon our notice. If there can be no knowledge of matters of intention, if scientific knowledge is the only form of knowledge which is possible to us, then all human life must be the sport of a blind chance. For scientific knowledge, being instrumental knowledge, has the effect of increasing

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almost without limit our power to realize our intentions. But whether it is employed for good or bad purposes, for construction or for destruction, to increase happiness or to increase misery, depends upon the character of the intentions which it serves. If there can be no knowledge which enables us to unify or harmonize the very many inconsistent intentions which human beings seek to realize, then there can be no way open to us by which we can control, to the slightest extent, our own destiny or the future of the societies to which we belong. Whether the result of science will be to increase the intensity and horror of an inherently meaningless existence or will lead to peace and the increase of human happiness, must be a matter of pure chance about which we can do literally nothing at all. A belief in progress becomes almost our *credo quia impossibile*, a blind faith with no shadow of rational justification. Even if by chance it happened that human intentions did, in fact, harmonize, we should still have no justification for any belief that the general intention which succeeded in harmonizing them would be realizable in a world of mere fact to which it could have no inherent relation.

For myself, I find such a conclusion not

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merely abhorrent, but completely absurd. The whole history of human society disproves it. If it is true, I cannot understand how human society, which is a unity of intention, could ever have been possible at all; nor how it could have been possible for any human wisdom to achieve the reconciliation of conflicting aims by providing for the necessities of human life, and its adaptation to its material environment. Some of the wisdom, however inadequate it may be, which has made this miracle possible, has been embodied in the religious traditions of mankind. I find it difficult to believe that the accompaniment of the decay of religion in Europe by the breakdown of social cohesion and the relapse into barbarism which we are now witnessing, is a matter of the purest coincidence.

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